ARROYO SECO

SCIENTIFIC AMERICAN SUPPLEMENT

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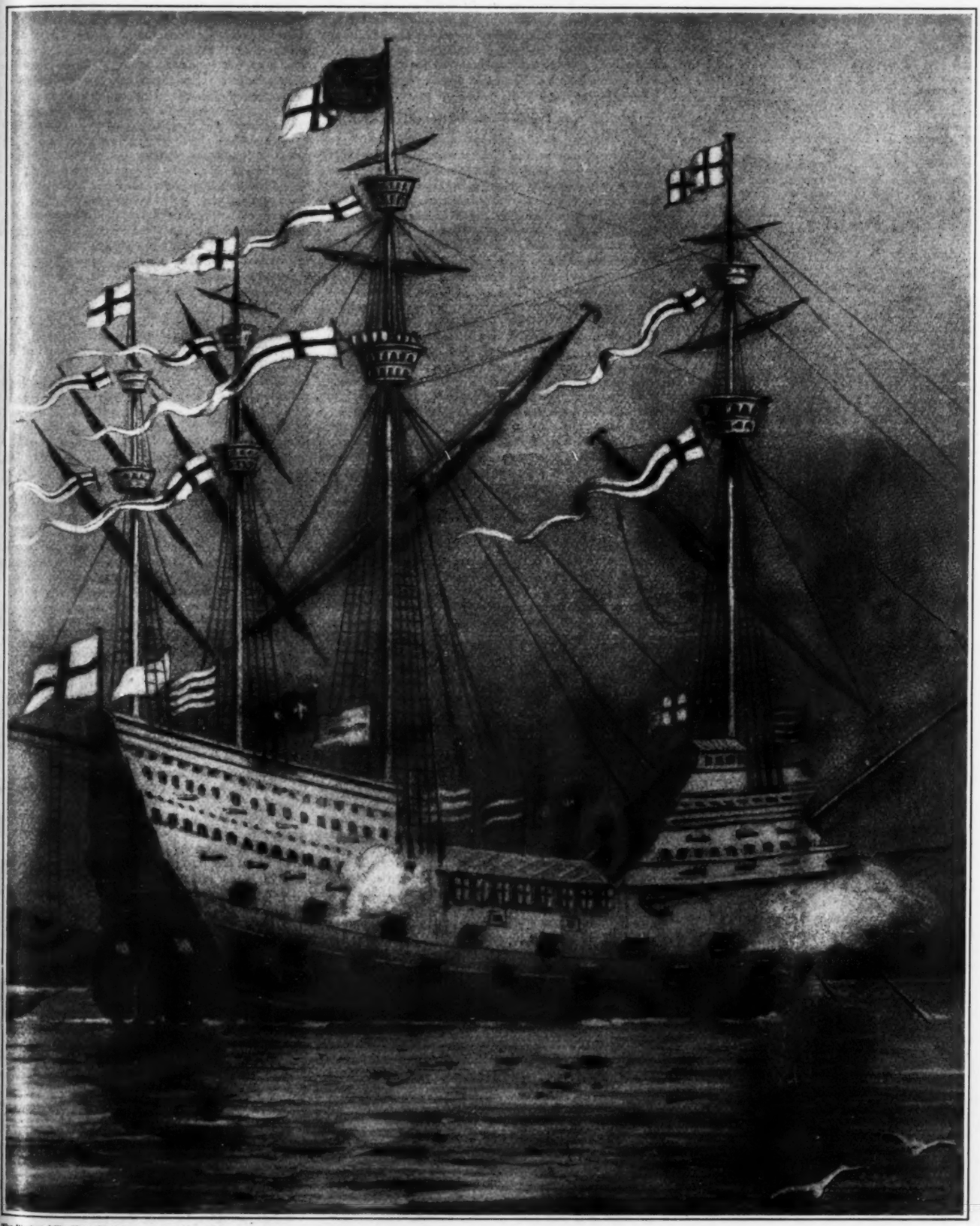
VOLUME LXXXIV]
NUMBER 2180

* NEW YORK, OCTOBER 13, 1917 *

[10 CENTS A COPY
$5.00 A YEAR



The Illustrated War News (London)

The "Great Harry" of 1514, the first dreadnaught, from a contemporary drawing in the Pepys Library at Magdalene College, Cambridge, England

WAR SHIPS OLD AND NEW [See Page 228]

Ultra-Violet Radiation[1]

In Relation to Treatment by Ultra-Violet Rays

By Charles A. Schunck, F.C.S.

As ultra-violet radiation is extensively used therapeutically I thought that a spectroscopic comparison of some of its sources, showing their constituent radiations, wave-lengths, and relative intensities, and the attempt to ascertain the most efficient for the purpose, would be of interest and value.

After describing the regions of the spectrum the following table was given:

THE SPECTRUM REGIONS

Visible region	7594-3934	A. U.
Ultra-violet region	3934-1850	"
*Schumann region	1850-1230	"
†Lyman region	1230-600	"
Unexplored region	600-8	"
X and Gramma ray	8-0.07	"

From λ 600 to about λ 8, where the softest characteristic X rays of Al have been recorded, is an unexplored region, the region where that profound change from light radiation to X ray takes place, and which, when means are devised for its investigation, may lead to most important results. Lyman considers it can be still further extended below λ 600 by purely spectroscopic means.[2] The wave-length of the soft X rays is of the order of 1 Angström unit and of the harder 0.25. The value 0.07 is for the most penetrating rays from radium C, given me by Sir Ernest Rutherford in 1915, who also gave his measurement of the hardest rays from the Coolidge tube as 0.14 A. U.

The Quartz Spectrograph, with which the spectra were recorded, is of well-known design and admirably adapted for the purpose.

The one completed for me last year by Messrs. Bellingham and Stanley, of 71, Hornsey Rise, has various improvements upon the original model of A. Hilger and Co., from whom I had one of the first in 1908. The salient features are its compactness, the complete enclosement of the prism, and lenses able to take the entire spectrum from the extreme red to the ultra-violet at λ 2100 on one plate 10 inches long in excellent focus throughout; when once set no further adjustment is required. [A description of the instrument was here given.]

For the arc spectra the voltage was 100, and the amperage was varied from 5 to 10. For each plate the amperage and exposures for each spectrum thereon were kept constant so as to get comparable results. The exposures in the arc spectra varied from five to fifteen seconds, whereas the corresponding spark spectra took 2½ to 5 minutes. The plates used were Wratten and Wainwright pan-chromatic and the sources were compared on the same plate. The various sources were examined both in the arc and spark discharge, though the latter is hardly applicable therapeutically in general, the spark only being a point of light compared to the arc, and to be at all practicable would have to be condensed and then but a very small area would be covered. This might be useful in some cases, however.

THE SOURCES OF RADIATION

The sources of radiation examined consisted of:

Metallic electrodes of pure[3] W, Mo and Fe; ¼ inch carbon rods impregnated by boiling in solutions of sodium tungstate, uranium nitrate, ammonium molybdate, and titanous chloride; and ¼-inch cored carbons filled with U_3O_8, Fe_2O_3, wolfram (75 per cent WO_3, 20 per cent FeO, 5 per cent MnO), the ore of tungsten, and pure W and Mo (99 per cent) in the form of powder, together with the Simpson electrodes which are said to consist of wolfram. These are very unsteady in the arc, splutter badly, and a glassy deposit forms on the electrodes that has to be removed before the current will pass again, whereas the above carbons are quite steady, which is a great advantage.

The reason for trying the impregnated and cored carbons was the readiness one can assimilate any substance for the arc discharge and the endeavor to get, if possible, a more intense and cheaper source in that manner than the Simpson's or metallic tungsten. From the spark spectra experiments I had high hopes that the carbons impregnated with uranium nitrate and ammonium molybdate[4] would prove an excellent source, and rival that of W, whose spectrum is of a similar character. But though it has a more intense spectrum than W in the spark and of greater extent, it falls off considerably after λ 2500 and ends at λ 2300, compared to W in the arc, which extends to λ 2200 on this plate. W also appears to be more intense in the arc than the spark.

THE SPECTROSCOPIC RESULTS

The following are the spectroscopic results obtained:

The most intense and richest source of ultra-violet radiations is that of the W metallic electrodes in the arc. The spectrum[5] consists of very many lines of nearly equal intensity throughout the ultra-violet spectrum, so close together as to form an almost continuous spectrum extending with but slight loss of intensity to λ 2130 (the limit of the instrument). The W spectrum and the Simpson electrodes extend further with the same amperage and exposure than any of the sources examined.

The Mo arc spectrum is of a similar character but not so intense throughout, extending to λ 2190 but losing in intensity at λ 2300. The Fe arc spectrum is very rich in ultra-violet radiations, and though containing some very intense groups of lines is not so uniform throughout as that of W and extending only to λ 2250. Its spark spectrum is characterized by a maximum of intensity in the neighborhood of λ 2400, which maximum λ 2500 is not so pronounced in the arc.

The spectrum of the Simpson electrodes is very similar in character and of the same intensity spectroscopically as that of the W arc, but their unsteadiness may detract from their effectiveness therapeutically as compared to metallic W electrodes with treatment exposures. Dr. Sidney Russ was the first to examine the spectrum and to compare it with that of pure W, and he pointed out and came to the conclusion that they were apparently identical, thereby rendering a signal service. The wolfram cored carbons have a similar spectrum, but it is not so intense with the same amperage and exposure. A careful comparison of the spectra of metallic tungsten, Simpson's and wolfram, shows that though very similar, supporting Dr. Russ's conclusion, yet there are differences from the W spectrum, and giving an increased amperage to the wolfram carbons, almost an exact match is obtained between the Simpson's and them.

Of the cored carbons the W powder gives a less intense spectrum than the W electrodes. Fe_2O_3 almost equal to the Fe electrodes with advantage of steadiness which the latter lack, as they scintillate considerably. Mo powder like the Mo electrodes consisting of many lines extending to λ 2300, but not nearly so intense. The U_3O_8 was disappointing, and better results were to be expected. The oxide, however, was not reduced to the metallic state by the arc discharge which may be the cause, and shortly I hope to be able to examine the arc spectrum of metallic uranium electrodes. The spectrum loses intensity at λ 2600, but, so far, the lines are much closer together than W, and the spectrum more intense. The numberless lines and their closeness, both in the arc and spark spectrum of U, is its great characteristic.

The impregnated carbons in the spark are much more intense than in the arc. The uranium nitrate and ammonium molybdate impregnation gives a magnificent intense, almost continuous spectrum extending to λ 2300 without much loss of intensity, and is the most intense and richest in ultra-violet radiations of any spark source. The carbons impregnated with sodium tungstate were not nearly so continuous or intense. In the arc as far as λ 2500 the carbons impregnated with uranium nitrate and ammonium molybdate are more intense and continuous than the W arc, but their spectra do not extend as far, λ 2300 being about the limit. The sodium tungstate and titanous chloride impregnation are particularly poor. The same impregnated carbons were used both for the spark and arc.

Radiations of the Sources Used in Treatment.—By comparing the arc spectra of carbon, the iron spark and arc, the mercury vapor arc, the Simpson electrodes and metallic tungsten in the arc, the nature of the radiations, and the intensity of the various sources that have been and are used in ultra-violet radiation treatment are seen.

Carbon is the source used in the Finsen lamp, the spectrum being very poor in ultra-violet rays and the exposure as long as 70 minutes at a time. After the Finsen lamp, I believe both the iron arc and spark were used, the latter with a quartz condenser with exposures of three minutes. Then the mercury vapor arc as in the Kronmeyer lamp, with a quartz condenser as a compressor on the skin, with exposures of three minutes or thereabouts. The spectrum consists of intense isolated bands and lines, the most intense and broad of all being just the Schumann regions at λ 1849.6. A year ago the Simpson electrodes were the vogue until Major Turrell adopted metallic tungsten, with which the maximum exposure is five minutes without a condenser.

In regard to filament lamps, like the spectrum of all incandescent solids, the spectrum which is a continuous one falls short of λ 3000, so they are hardly suitable for ultra-violet radiation treatment where radiations of shorter wave-length are the effective ones therapeutically, as I believe I have been able to prove.

THERAPEUTICAL EXPERIMENTS

Through the valuable advice and assistance of Major W. J. Turrell, R.A.M.C., electro-therapeutic physician to the Radcliffe Infirmary, Oxford, to whom I am much indebted for the therapeutical aspect of ultra-violet radiation, I have been able to make some simple therapeutical experiments of the more intense sources.

Major Turrell[6] finds that the W arc (he was the first to apply this source), is the most efficient source clinically of ultra-violet radiation and superior in results obtained from the Simpson electrodes, which is supported by the spectroscopic examination. Within certain limits, he states, the intensity of the erythema produced on the skin in a given time may be taken as measuring the clinical efficiency of the radiation of the source. The open W arc he places one foot from the part to be treated, giving an exposure of five minutes with an amperage of five to ten, voltage 100, as the maximum dose.

The following experiments were undertaken:

In conjunction with him my forearm was exposed for five minutes at a distance of 12 inches with a current strength of five amperes over different areas of the skin to the action of the electrodes in the arc, of metallic W, Simpson's and half-inch cored-carbons filled with U_3O_8, W powder, wolfram and Fe_2O_3. The cored-carbons, which burned very steadily, gave no erythema. The W a fair reaction and the Simpson's slight in 12 hours; the former gave a slight redness in two hours, the latter very slight in the same time.

To get an increased effect I used the quartz cylindrical condenser under the same conditions as the spectra were taken, which forms an image of the arc 16 inches from the condenser in the form of a band about one-half inch wide on the skin of the forearm, the arc source being 3¼ inches from the condenser. The current strength, voltage 100, was nine to 10 amperes and the exposures about two minutes. An intense erythema was produced in a few hours with metallic W (indication in half an hour), not quite as much with the Simpson's (indication in one hour), and fairly intense with metallic Fe electrodes and the carbons cored with wolfram (indication in two hours). The exposure in this case was 2½ minutes, amperes 10. Using W as one electrode and a soft-cored carbon the other, alternately as "positive and negative," compared to W as both electrodes, there appeared but little difference in the erythema produced. With W as the positive electrode the arc is much the steadiest, and very steady, indeed. The three spectra at nine amperes and 10 seconds' exposures showed very little difference in intensity, W the best, then W as the positive electrode. Carbons impregnated with uranium nitrate and ammonium molybdate, nine amperes and four minutes' exposures, gave a faintish erythema in 24 hours, but no reaction with an exposure of two minutes.

One mm. thickness of glass in front of the condenser with an exposure of four minutes at nine amperes gave no reaction to the W arc, neither was there a reaction with a piece of microscopic cover-glass in front, whereas the W arc with 2 minutes' exposure gave a good redness

*Schumann substituted fluorite for quartz and adopted a vacuum spectroscope to avoid absorption of the rays.

†Lyman in 1914, using a concave grating (dispensing with necessity of lenses) enclosed in a brass tube 11 cm. in diameter and over a meter long containing hydrogen at a pressure of 2 to 3 mm. and using a strong disruptive discharge through a discharge tube, was able to extend the spectrum to λ900; and in 1915, by substituting He free from N for H and giving an exposure of about 10 minutes, repeatedly observed a number of new lines, the most refrangible of which was λ600.

[1] Abstract of a paper read before the Röntgen Society, reported in *The Lancet*.

[2] Theodore Lyman; The Extension of the Spectrum beyond the Schumann Region, *Astrophysical Journal*, March, 1916.

[3] W = Tungsten, Mo = Molybedenum, Fe = Iron, Ti = Titanium, U = Uranium, Mn = Manganese, C = Carbon.

[4] Known as the "Jones electrodes, and used as a source of light for recording absorption spectra in the ultra-violet, as the lines are so numerous and close together as to form almost a continuous spectrum.

[5] Shown at a meeting of the society last year by Messrs. Morphy and Mullard in their paper on "The Enclosed Tungsten Arc," and also by Major Wilson, C.A.M.C., when describing "a new tungsten arc lamp" he has devised.

[6] Ultra-violet Radiation from the Tungsten Arc, Section of Electro-Therapeutics of Royal Society of Medicine, Oct. 20th, 1916; *The Lancet*, 1916, ii., 790.

on the skin in 12 hours. These cover-glass experiments were repeated several times, and there was no difference if the glass was placed on the skin. Only in one experiment was there a slight reaction with 2½ minutes' exposure and 10 amperes, and in this experiment I am not certain whether the glass was covering the lens completely. The one mm. glass and the cover-glass tested spectroscopically under the same conditions as the erythema experiments were made, cut off the spectrum at λ 2950 and λ 2750 approximately.

This shows, I think, fairly conclusively that radiations of λ 2750 approximately and upwards have no therapeutical value as judged by the erythema effect, whereas if the one experiment is correct there is an effect between λ 2750 and λ 2950. It must be remembered these experiments were only made upon my own skin; still, an exposure of four minutes to the W arc with the condensing lens is a very severe test, seeing that in the arc experiment of 2½ minutes at 10 amperes the erythema was so intense that it persisted for three weeks, the skin desquamating and causing a fair amount of irritation.

Dr. Sidney Russ[3] last year stated that the skin is very absorbent to A. U. radiations.

A piece of skin from the human abdomen 1/10 mm. thick placed in front of the slit of the spectrograph with the Simpson arc 20 cm. distant and giving an exposure of two minutes, cuts off the spectrum approximately at λ 3000, showing that ultra-violet radiations between λ 3000 and λ 2100 are very easily absorbed by the human skin, and this is the *same* region I find of therapeutical value. "With thickness of ½ mm. and 1½ mm. of skin and an increased exposure of eight minutes the radiations between λ 3000 and λ 3800 were transmitted, but he considers it is doubtful whether more than 1 per cent of the radiation penetrates as deep as one mm."

These two sets of experiments tend to show that the therapeutical effect is limited to the superficial tissues, and this is also the opinion of Major Turrell from the clinical aspect, who states in the paper above mentioned that "the therapeutical value of ultra-violet radiation appears to be due to its destructive action on micro-organisms and to the active hyperæmia which it induces in the superficial tissues."

I have also proved while constantly working with the W arc in this research, that a sheet of 1½ mm. glass placed between the arc and the face is a perfect protection from the effects of the radiations, especially to the eyes, which are particularly sensitive, which is supported by my own experience, my eyes being much affected before adopting this means of protection. During one day's experiments I calculated I was subjected for half an hour to the W arc one foot away, and with this simple protection there was no effect on the eyes or skin.

I think one can safely assert that the radiations of therapeutic value lie between λ 3000 and λ 1850, and I am now endeavoring by spectroscopic means to ascertain which part of this region has the greatest therapeutic efficiency. The W arc is no doubt the most intense source of radiation there is at present for the purpose. The open arc can be increased in intensity by using a quartz condensing lens, the absorption of which for these radiations is but slight. Pflüger for a piece one cm. thickness gives the values of 5.8 per cent at λ 2200, 8 per cent at λ 2140, 16.4 per cent as λ 2030, but as much as 32.8 per cent as λ 1860; while Collentz finds that throughout the spectrum to λ 2500 it is almost perfectly transparent in thicknesses up to 3 cm. If a reflecting mirror is used,[4] then platinum and nickel will be found superior to silver, and silicon the best of all reflecting surfaces. Silver reflects on an average 30 per cent of the radiations between λ 3000 and λ 2000, platinum and nickel about 45 per cent, but silicon as much as 76 per cent. Mirrors of these substances can be readily prepared by cathode spluttering or electro-plating.

[3] From the Visible to the Gamma Ray Spectrum, *Brit. Med. Journ.*, Jan. 22d, 1916.

[4] The reflecting power of metals in the Ultra-Violet Region of the Spectrum, by E. O. Hulbert, *Astrophysical Journal*, 1915, xlii, 205.

Rainfall and Gunfire

M. Angot, the eminent director of the French Meteorological Service, has made a valuable and authoritative contribution, published in the Journal of the French Academy of Agriculture for May, to the literature of a well-worn controversy. The alleged connection between rainfall and gunfire, in favor of which so many champions sprang up during the wet periods of 1914–16, has recently lost favor as a subject for argument, owing, no doubt, to the coincidence of the spring drought of 1917 with the Allied offensive on the Western front; but so short is the public memory, especially for negative evidence, that the incidence of three inches of rain during a recent summer afternoon in North-West London has proved sufficient to disinter the bone of contention. The mental attitude of the public towards a theory of this nature is of great psychological interest: there is little doubt that, should we experience this summer a repetition of the weather of July, 1888, when snow fell in London, followed by a recurrence of that of August, 1911, when the thermometer touched 100° F. at Greenwich, both phenomena would generally be attributed to the war.

Accordingly M. Angot's paper reaches us at an opportune moment. After dealing briefly with the historical aspect of the question, and alluding to the work of M. Le Maout—who, not content with having established a connection between the bombardments of the Crimean War and the rainfall of India, the United States, Nacaragua, and Barbados, went on to ascribe the diurnal variation of the barometer to the striking of public clocks and the ringing of chruch-bells—M Angot proceeds to consider the physical changes which could be effected by the discharge of artillery, and could at the same time be held responsible for the causation, increase, or acceleration of rainfall.

The first proposition is that a succession of violent explosions might result in the displacement of masses of cold air at certain heights, which, coming under the influence of the upper winds and encountering layers of warmer, saturated air, could give rise to precipitation which would not otherwise have occurred: in this connection the author points out that in order to obtain a rainfall of so small an order as 1 mm. (0.04 in.), even if one were to take two equal masses of saturated air, the one at a temperature of 0° C., the other at 20° C. (an extreme case, of course), it would be necessary to effect a rapid and thorough intermingling of the two throughout a layer of air 6850 metres in thickness. In M. Angot's opinion, the mixing of layers of air may be the cause of cloud-formation or of slight drizzle at the earth's surface, but can never be responsible for considerable precipitation.

In the case of the second proposition—that water-vapor resulting from chemical reaction of the explosives might take effect—it is asserted that in order to produce the same amount of rainfall (1 mm.) as in the previous proposition the employment of no fewer than 21,750 tons of melinite per square mile would be necessitated—that, indeed, only on the supposition that all the hydrogen in the explosive became water-vapor which condensed immediately in its entirety and, so to speak, on the spot.

In the third and last instance, the possibility of electrical action being brought into play is considered in some detail. We know that supersaturated air (*i.e.* air which contains more water-vapor than it normally should be able to hold for the existing temperature) is a physical possibility, in the absence of dust-particles or other matter which may form nuclei for condensation. The necessary medium may be supplied by the action of ozone, of ultra-violet rays, by any cause, in fact, which can set up ionization of the atmosphere; under this last category may be classed the detonation of high explosives, inasmuch as highly ionized gases result therefrom. The lower regions of the atmosphere, however, which alone are the seat of explosive activity on a large scale, always harbor large numbers of both ions and dust-particles, and cannot, therefore, be subject to supersaturation; while it has yet to be shown that the addition of quantities of ions or of dust-particles to a stratum of atmosphere nearly, but not quite, saturated can bring about premature condensation. Assuming for the moment the possibility of such a hypothesis we must consider that no outpouring of ions or dust-particles can do more than accelerate a precipitation which would be necessitated sooner or later by the progressive cooling of the air, since the mass of water that results from the cooling of, say, a kilogram of saturated air from 15° C. to 0° C. is constant (rather more than 5 gr.), whether or not supersaturation may have existed at the inception of the temperature-reduction.

Having thus pronounced upon the theories which have been advanced to account for the alleged connection, M. Angot goes on to consider whether in reality anything has occurred that needs accounting for—whether the rainfall since the outbreak of hostilities has been less inclined to observe the rules by which we endeavor to forcast its occurrence than before. Careful comparison between the daily weather-maps and the observed rainfall figures has convinced him that it is not. He points out, very rightly, that we have been passing through a series of wet years since 1909—a period that balances the run of dry years 1898—1904 (1903 and 1911 were both exceptions to their groups and may be said to balance one another)—and that excess of rain in 1915 and 1916 might reasonable have been expected; that 1909 was wetter (in France) than 1915, 1910 than 1916; furthermore, that during December, 1915, an unprecedentedly wet month, relative calm prevailed over the whole front, and that in the second ten-day period of the very wet February of 1916 considerably more rain fell (40 mm. as against 28 mm.) than in the last ten-day period, which witnessed the development of the giant German bid for Verdun. Similar conclusions will be reached if frequency of rain instead of amount be considered: 1910 had more rain-days than 1916; 1912 and 1913 both more than 1915, when the number in France was eleven below the average. The author has found nothing exceptional in the local distribution of rainfall: proximity to the firing-zone has not resulted in relatively greater totals or frequencies, while the great spring offensive of 1917 failed to interrupt the long spell of brilliant weather which accompanied it.

An examination was made some months ago at the British Meteorological Office into the local distribution of rainfall over England during the first twenty-two months of the war, the results of which afford corroborative evidence for M. Angot's last-mentioned point. It was found that the greatest excess of rain over the normal figure was one of 59 per cent, on the South Yorkshire coast; that three areas in Lincolnshire and on the Norfolk and Suffolk coasts respectively had rather more than 40 per cent excess; but that round the North Foreland there was a slight deficit. No trace whatever of a distribution having reference to a centre over northern France was discoverable.

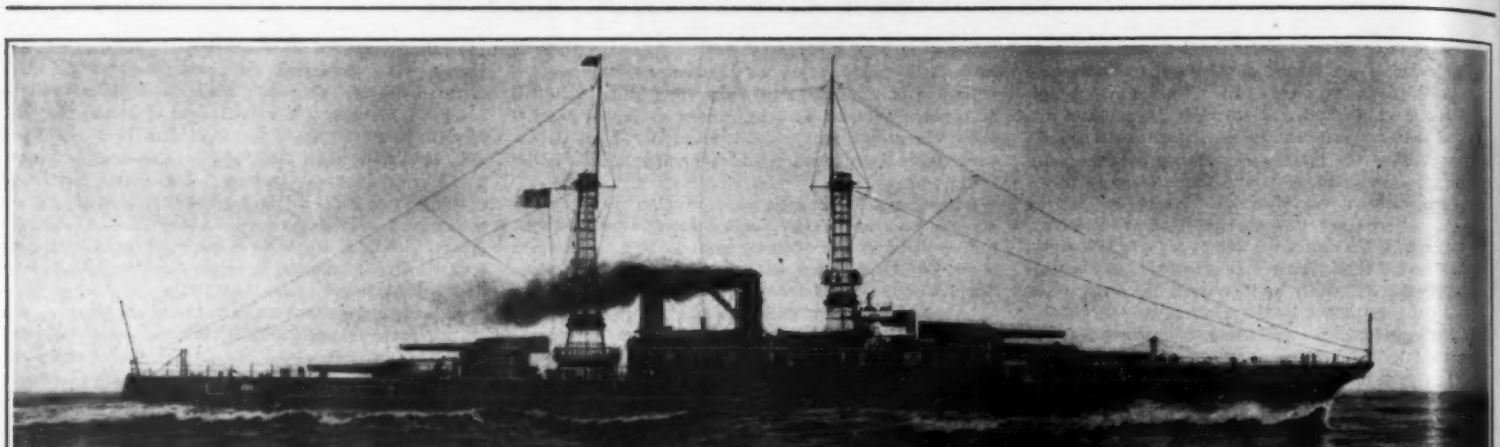
M. Angot concludes with the reflection that it may be with rainfall and gunfire as it is with weather changes and the phases of the moon, that "sous la suggestion d'une croyance instinctive on est conduit involontairement à ne remarquer que less coincidences favorables et à s'affermir ainsi de plus en plus dans cette croyance." For those, indeed, who are cognisant of the relationship between the weather and modern warfare it is not difficult to see the possibility of the connection, but it is a connection in which the amount of gunfire varies inversely as the amount of rain that is falling rather than one which makes the rainfall in any way dependent upon the gunfire.—E. L. Hawke, in *Nature*.

Soil Solution Obtained by the Action of a Hydraulic Press

The authors point out that the analysis of drainage waters from soils does not afford an accurate means of determining the composition of the normal soil solution, as drainage only occurs when the soils are supersaturated. For this reason they adopted the method of forcing water out of the soil with a hydraulic press. Samples of 3 kilos. of soil were taken from the fields and subjected to a pressure of 300 kilos. to the square cm., the liquid expressed being then analyzed for calcium, magnesium, sulphates, phosphoric acid, and potassium. The sampling was done on six different occasions over a period lasting from May to October; both surface and subsoil were used. The calcium content was found to vary considerably in the surface soil, but in the subsoil it seemed fairly constant, except for a rise in midsummer. Potassium, contrary to the generally accepted view, behaved very much like calcium, that is, its content fluctuated according to the general concentration of the soil solution, rising when evaporation took place and being lowered by spells of wet weather. Further, there was evidence of potassium and calcium being transported from the subsoil to the surface during a prolonged period of drought, but no evidence was obtained that adsorption exerted any regulating effect on the concentration of the soil solution. The exchange of bases only occurred when the proportion which the dissolved substances bore to one another was altered. The authors suggest that the selective action of the plant roots, by throwing the soil solution out of equilibrium, would have a considerable effect in bringing fresh supplies of nutrient substances into solution. They state also that the pressure method of obtaining soil water is only applicable in the case of soils made up of very fine particles or containing a considerable amount of humus.—Note in *Journal of the Society of Chemical Industry*, on a paper by G. Ramann, S. Marz and H. Bauer in *Int. Mit. Bodenkunde*.

Removing Rust by Electricity

An electrolytic process of deoxidation has been patented in the United States. The object to be treated is made the cathode in an electrolyte containing phosphoric acid. In addition to its normal function of carrying the current, this acid acts as a solvent upon rust without attacking the steel or iron body beneath. It is in this last detail that its chief availability lies, since nitric, sulphuric, or hydrochloric acids would not display such moderation. Finally, the phosphoric acid is beneficial in preventing subsequent further rusting. The electrolyte is made by adding 10 parts of phosphoric acid to 90 parts of water, or by adding 10 per cent of the acid to a 10 per cent solution of sodium phosphate. A temperature between 50° and 70° Centigrade is recommended.



The "Colorado," of the U. S. Navy. The latest development in battleships

War Ships Old and New

Changes that Surpass the Wildest Flights of Fiction

We are all so familiar, through reports and pictures, with modern methods of warfare that there is little appreciation of the stupendious engines of war that are employed by every great nation to protect their territories to uphold their rights, or even to impress their interests on their neighbors, and this is particularly true in regard to the ships composing the navies of the world. A fair conception, however, of the advances that have been made in recent years can be gained by a comparison between a war ship of the olden time and the latest type of vessel being built in this country, and for this purpose pictures are herewith shown of what has been termed the earliest "Dreadnaught" and a modern high-powered battle ship, On the first page of this issue is shown a picture of the Henri Grace a Dieu, or the "Great Harry," as it was usually called, which was built by Henry the VIII of England, in the year 1514, while at the top of this page is shown the latest American design, which is the most powerful yet proposed.

It may first be well to explain what the term "Dreadnaught," which is so frequently employed in descriptions of fighting ships, means, and its derivation. For many years the procedure in naval battles was for the two fleets to get as close together as possible, and then to hammer each other with every gun available, each side endeavoring to overwhelm its opponent by a tornado of shot and shell. Students of naval science in England, however, after a careful study of the results of naval actions of the Japanese fleets with the Russians and the Chinese, and also between the American and Spanish ships, decided that chance played too great a part in this style of action, especially as the power at such short ranges of guns surpassed the protective strength of any armor that could be carried—the solution of the problem arrived at was to build a ship, on which practically the entire armament should consist of big guns, thus enabling it to take up a position out of range of ordinary guns, and bombard the enemy at its leisure. The vessel built to carry out this idea was the Dreadnaught, which was launched in 1906. This ship carried ten 12-inch guns, the only other weapons being a number of small 12-pounders for repelling torpedo boat attacks. The other element entering into the plan was speed, to enable the ship to maneuver as it desired, and the Dreadnaught was equipped with engines of 23,000 horse-power to give it a speed of 21 knots, which was unusual at that time. This ship was such a departure from previous theories that its name has been used ever since to designate vessels of that type, although the idea has undergone many modifications. As the Dreadnaught surpassed other existing ships in power, so it too was soon superseded by still more formidable craft, and these have been called "Super-Dreadnaughts," although this latter term is now no longer as distinctive of a type as the former was for a long time.

Returning to the "Great Harry," this ship was built of wood, and was of 1,500 tonnage, so it was probably not far from 200 feet long. But its claim for distinction was that it was the first ship built to carry guns on two or more decks, and the first to fire through portholes. Before that the guns were fired over the rails, but in the new ship, the first "Dreadnaught," the crew were afforded the protection of the bulwarks and sides.

The "Great Harry" carried 184 pieces of ordnance, 34 being culverins and demi-culverins, and she also had over 120 brass and iron serpentines. Her other armament consisted of 2,000 bows for archers, 1,500 each of bills and morris-pikes together with other hand weapons. Her crew numbered 260, and she carried 400 soldiers, or marines as we now term them.

The culverins here mentioned were guns having a bore of about 5¼ inches, were 11 feet long and fired a round shot weighing about 17 pounds, fired by a charge of 12 pounds of very poor powder. The demi-culverins were about 8 feet long with a bore of about 4 inches, and threw a shot of 8 or 9 pounds with a charge of 6 pounds of powder. The serpentine was the largest gun of all, having a bore of about 7 inches, and it used 25 pounds of powder to throw a shot of between 40 and 50 pounds weight. Owing to the imperfections in workmanship in both guns and shot, and the poor and uncertain quality of the powder of those days the range and power of the guns was very limited, so that shot fired at close range frequently failed to penetrate the wooden sides of the ships struck, and naturally the accuracy of fire was very low.

The "Great Harry" was a show ship, and was elaborate in her decorations and fittings, indeed one writer describes her as a mass of millinery and gilded timber. Some say her sails were of damask, and the illustration shows how she was adorned with silken banners. Her architecture was unique, and a curious exaggeration of the ideas of her time, and it is not surprising to learn that she was found to be a very poor sea boat. Indeed it is said she was never put to any practical use, and was, in fact, but the demonstration of a theory; nevertheless it inaugurated a new era in the designing of warships.

Skipping some four hundred years we present on this page a drawing of one of the most modern of battleships which is believed to combine in the best manner all of the latest ideas for a sea fighter. This is the "Colorado", designed for the navy of the United States, and from these plans four ships are to be built. The "Colorado" is a most impressive contrast to the old wooden frigate in every way. Being built of steel, it is possible to construct a much larger, as well as stronger ship than would in any way be possible with wood, so we find this latest ship is no less than 624 feet long, 97 feet wide and it draws 30 feet 6 inches. Probably the tonnage figures best illustrate the great contrast between the past and the present, for the "Colorado" will be of 32,600 tons as against 1,500 tons for the "Great Harry," or more than twenty-one times bigger. For armament the "Colorado" class will carry eight guns 60 feet long, with a bore of 16 inches, which can throw a shot weighing 2,400 pounds a distance of about 27 miles. To propel this immense mass of metal a charge of 900 pounds of powder is burned; and when we consider the difference in power developed by the powder of today, as compared with that used 400 years ago, it is safe to say that one shot from one of these 16-inch monsters would result in more power than would be developed by the hundred odd guns of the "Great Harry" in a long engagement.

It is interesting, while making comparisons, to refer to some of the characteristics of the original Dreadnaught, as the "Colorado" is usually referred to the same family of fighters. One of the features of that vessel was superior speed, to enable it to pick and choose its position with reference to the vessel attacked, and she was capable of 21 knots, being driven by engines of 23,000 horse-power; but in the "Colorado," and for that matter in the battleships of other nations also with few exceptions, the speed has remained at 21 knots, with 26,800 horse-power. This is because of the difficulty in providing space for machinery and fuel, for progress in this direction has become slow.

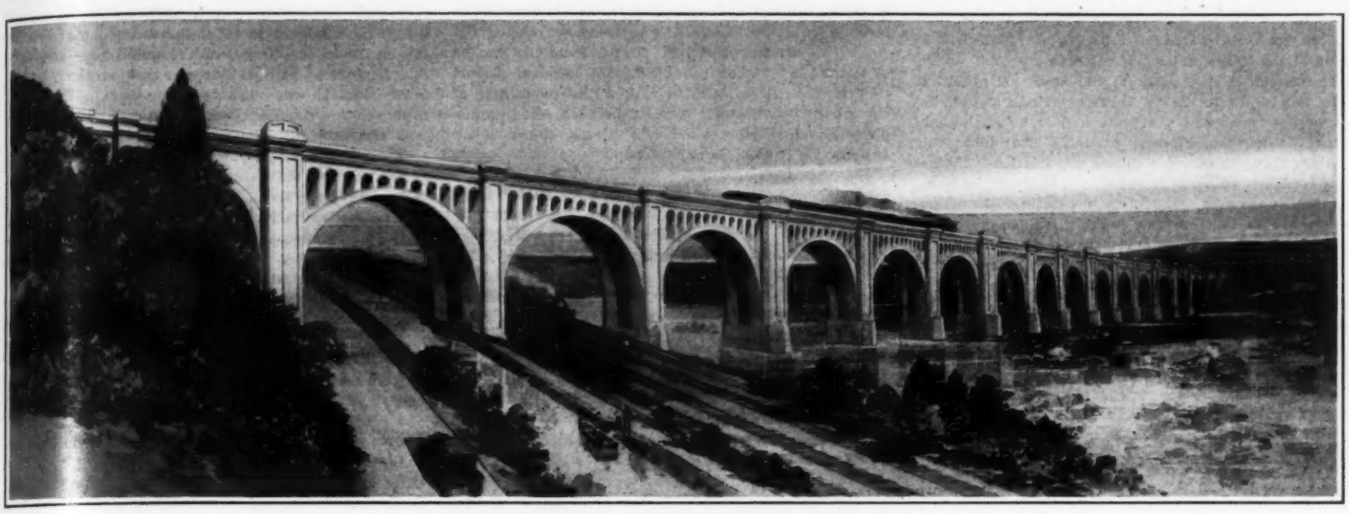
In the matter of secondary battery power the "Colorado" class follows the theories inaugurated in the Dreadnaught, although it has been found desirable to make the light guns carried of great power, and the "Colorado" class is equipped with twenty-two 5-inch guns. It also has four 3-inch anti-aircraft guns and two 21-inch torpedo tubes. The crew of our latest fighting ship will number 1022 officers and men.

The building of more war ships is deprecated in some quarters on the theory that after this war there will be no need for them, and that universal peace will prevail throughout the world. History shows no grounds for such a belief, for it has been demonstrated by many hundreds of years of experience that treaties are of little effect for accomplishing such a purpose, and until the nature of man changes radically, of which there is no indication at present, there will be wars. We are justified in believing that race prejudices will continue to exist; moreover, the struggle for existence, between individuals, communities, states and nations constantly becomes more strenuous with increasing population, and under such conditions it seems impossible that serious conflicts of interests will not occur. One possible solution may be possible, and that is the universal commingling of races, as is the case in this country; but the conflicting interests of geographical divisions still remain.

A mere glance at the two illustrations herewith given strongly impresses the observer with the almost miraculous progress that the world has made, and is still making. When the "Great Harry" was built the steam engine was unknown, nor was the use of iron and steel for constructing ships dreamed of. Guns and gunpowder were of the most primitive character, and a single modern torpedo boat destroyer could put an entire fleet of those crude and unwieldy battleships to flight. The interior fittings and mechanism of a modern battleship offers another wonderful contrast, for the electrical equipment alone would have condemned the ancient designer who suggested such possibilities to the stake as a dealer in the black arts. What modern invention might lead to no one can foretell; but it is to be hoped that the results of the present war may tend to eliminate the building of these mighty engines of war entirely.

Discovery of Prehistoric Bronze Weapons

While the wastage of the Yorkshire cliffs is to be deplored, the result is sometimes of advantage to the geologist and antiquary. Recently, in the vicinity of Scarborough, a fall of the cliff has revealed a hoard of 20 bronze weapons, which consisted of battle-axes, spears, chisels, gouges, portions of a sword, etc. Twelve of the axes, of the socketed type, are perfect. One shows the unusual feature of a rivet-hole in place of a loop for secure hafting; another contained a portion of the original wood shaft. Some of the axes are in the rough state, as if just turned out of the mold; others have obviously been in use. The collection evidently formed the stock-in-trade of a metal-worker of the Bronze Age, at least 1,000 years before the Christian era.—*Nature.*



A new concrete bridge across the James River, at Richmond, Va.

The James River Bridge

THE accompanying illustration shows the design of the James River concrete bridge at Richmond, which indicates the advantage of concrete over steel railroad viaducts.

On account of the low cost of the concrete aggregates, furnished by the railroad, the bid for a concrete arch viaduct 2,278 ft. long was nearly $15,000 less than for an alternate steel design of plate girders and steel towers on concrete piers. This bridge replaces the single-track steel structure at the Belt Line crossing of the James River in Richmond by the Richmond, Fredericksburg & Potomac R.R. and the Atlantic Coast Line R.R.

The new double-track structure is being built parallel to and 30 ft. upstream from the old bridge and the new concrete structure is designed for the equivalent of Cooper's E-60 loading. The river bed is of granite, and a solid foundation is secured with comparatively little excavation. The maximum river stage is 11 ft. above mean water, while at times a large part of the river bed is exposed!

This location seemed well suited to concrete arch construction, but alternate designs were made by the consulting engineers for a concrete arch bridge and also for a steel viaduct with plate-girder spans, supported on steel towers resting on concrete piers.

A comparison of the bids for concrete and steel structures is of interest.

Concrete arch.		Steel viaduct.	
Bids to subgrade..	$416,122	Bid to subgrade..	$444,885
Ballast and ties...	5,520	Track	7,950
Rails	6,532	Approaches	49,600
Approaches	49,600		
	$477,774		$492,435

It may be stated that these prices were based upon a very low cost for aggregates, as the railroad company agreed to furnish sand and gravel for concrete at 60c. per cubic yard. The commercial prices per cubic yard of these aggregates at Richmond were $1.05 for sand and $1.20 for gravel. The concrete arch bridge contains 41,400 cubic yards of concrete and the steel structure 3,350 cubic yards.

In order to make a comparison of figures that could have been expected had the contractors furnished sand and gravel at market prices, there should be added to the above totals $30,900 for the concrete arch design and $2,530 for the steel design, thus making the cost of concrete arch $508,674 and of the steel viaduct $494,965, and this may be taken as a fair comparison of the relative costs of the highest types of design. The concrete bridge has beautiful lines and the advantages of a ballasted track, low maintenance and long life, as against a plain and less satisfactory design in steel with open floor, higher maintenance and shorter life. The contract was awarded for the concrete arch bridge and it is now being built.

This concrete structure consists of a bridge 32 ft. 6 in. wide and 2,278 ft. long, including abutments, and the base of rail is at elevation 100 ft. above the river level, which is taken as datum. There are twelve arches of 116 ft. clear span and 58 ft. rise, three arches of 122 ft. clear span and 58 ft. rise, three arches of 60 ft. clear span and 30 ft. rise, and 70 ft. of abutment approaches.

The main span piers are 14 ft. thick springings, with an abutment pier 20 ft. thick for each group of three arches. The abutment and the 60 ft. arches have solid earth-filled spandrels, while the main arches are of open spandrel design with slab floor system. The main arches spring from elevation 21 and are full centered and concealed expansion joints are provided for the structure above the arches at each pier throughout the bridge. No hand rail is used, but refuge bays 5 ft. wide are provided over each abutment pier on both sides of the bridge.

The upper part of each pier has a pediment which, together with the refuge bay treatment, breaks the straight line of the coping, giving a pleasing, artistic effect. All surfaces of the concrete are reinforced to prevent face cracking, and the reinforcement of the whole structure is in accordance with the standard specifications of the engineers, allowing 16,000 lbs. per sq. in. tension in steel and 650 lbs. per sq. in. maximum compression in the concrete. The exposed surfaces are given a spade finish, secured by using dressed-lumber forms and spading the concrete during placing to secure a dense mixture of mortar near the form surface and all arches and walls are provided with drainage and are waterproofed on the inside. The photograph and data relative to the bridge were furnished by Engineer H. G. Perring of Baltimore.

Model of Largest Copper Mine in the World

IN Bingham Canyon, Utah, near Salt Lake City, is the largest and most remarkable mining operation in the world. Here where infrequent rains once washed furrows into the sage-covered slopes of the Wahsatch Mountains, a half hundred hungry steam shovels are now eating their way into the heart of a mountain of copper ore—not copper ore as ordinarily seen, rich and heavy, in conspicuous seams and veins; but a new sort of copper ore, rock with such a scant scattering of copper minerals that to the ordinary eye it looks no more like ore than common granite, rock so lean in copper values that to old-line mining methods it remains as unprofitable to mine as common granite. Yet this new type of copper ore, developed not only at Bingham, but recently also as a result of its leadership at several localities in the Southwest as well as in Mexico and Chile is now producing more copper than the whole United States provided a decade past.

Here is an object lesson of the present, a vision of the future that all should see. It is now available for all to see, for a model reproduction, larger than an ordinary room, true in detail as well as in general effect has been completed and installed in the Division of Mineral Technology of the United States National Museum. This model has been prepared with faithful elaboration because it represents not only a unique application of the relatively new open-cut, steam-shovel type of mining operation, but also a specific and important source of the essential metal copper.

This achievement of adding millions of tons of copper to the world's output is due to the vision and constructive imagination of a single mind. Twenty years ago the mining of rock in which the copper minerals could scarcely be seen was undreamed of, But one man dreamed—and acted; and the dream came true. He is Daniel C. Jackling, first vice-president and managing director of the Utah Copper Company.

In 1898 Bingham Canyon was a district of small and none-too-flourishing gold mines. Low-grade copper ores had long been known to be present, ever since, in fact, the Third California Infantry, stationed at Salt Lake City in 1863, varied their activities by prospecting in the neighboring mountains and drove a tunnel into the copper-bearing rock. But rock carrying in each ton only thirty pounds of copper, and that in the form of complex minerals, had to await large-scale engineering ideas to materialize. Jackling made an examination of this ungainly copper occurrence in 1898. He saw its possibilities. He reported that millions of pounds of copper were scattered through a mountain of rock and that the copper could be secured at a profit if operations were planned on a large enough scale; in other words, if the whole mountain were demolished in the process. But the idea was staggering, and not until five years later did capital venture into this tremendous undertaking. Success has now crowned the venture, not merely material or financial successes; but success of deeper significance. We have here the turning point between the mining of the past and the mining of the future. The bonanza type of mine, in which rich values are gotten from the earth with little effort, is fast becoming obsolete, even in the undeveloped regions of the globe. The world must turn more and more to low-grade ores for its essential metals, and the achievement at Bingham has blazed the trail.

Bingham Canyon is easily reached from Salt Lake City. After skirting the spurs of the Wahsatch Range and passing the enormous mills and smelter at Garfield, placed at the mouth of the Canyon twenty miles from the mine to handle the thousands of tons of ore that come down daily, the visitor's train turns into a narrow valley. Near its head, he dismounts in a mining town, pressed between precipitous slopes, smoky and noisy from passing ore trains. If he climbs the steep slope surmounting the place, he can gain a panoramic view of the mining activities. He sees a whole mountain, resting against the crest line of a snow-clad range, stripped bare of its mantle of soil, raw and naked in its demolishment. Terrace after terrace of gray rock, twenty-five in all, rise steplike from the valley bottom, and extend like giant stairs to the very summit, supporting nearly fifty miles of railroad. As the eye wanders up to the top it catches here and there puffs of white vapor, accompanied by raucous noises, where ponderous steam shovels, looking tiny in the distance, are scooping out tons of copper rock and loading their charges into long trains of cars gaping for their burden. It is ugly, yet fascinating, this unwilling tribute yielded up to the masterful effort of modern industry.

A City of Gardens

EVERY city of homes—and that means every city in at least part of its area—should be a city of gardens. People should be taught how to make it so, how little it costs to make it so, and how certain are the garden crops when properly irrigated. They should be led to realize that vegetables and berries plucked fresh are infinitely more palatable than the inevitably stale stuff that the markets provide.—*Engineering and Contracting.*

Adaptation and Disease[1]

THE time has come to bring before biologists in general the contributions of medical research of the last quarter of a century to the study of evolution. The fact of evolution all thinking minds accept, but as to how evolution has been, and is being, brought about is a very different matter. The fight truly centers upon the cause or causes of variation—whether the tendency to vary is something inherent in living matter, numerous variations presenting themselves through this inherent tendency, of which those that are best fitted for their environment alone survive, or whether it is primarily and essentially brought about by forces acting from without upon a relatively labile living matter: whether, that is, variation is inherent, proceeding from within, or acquired, proceeding from without.

But this basal problem has been largely neglected by the biologists, the fight all these years waging round the secondary problem of the transmission of acquired properties to the offspring. Herbert Spencer made this transmission of acquirements one of his "principles." Weismann violently[2] opposed the doctrine, carrying with him latter-day biologists, until Mr. Bateson, replete with his studies upon Mendelism, reaches the antipodal suggestion that when a new property manifests itself in any individual of any species, it is impossible to regard it as an acquirement: it is not new, but its manifestation is due to loss of properties already possessed. Evolution, like a squid, progresses backward, what appears to be a new property is on the contrary primeval. That which to outward seeming is the simplest form of life is verily in constitution the most marvellously complex: the higher forms of life are the lower.

The truth seemed to be that valuable and fascinating as are the studies for the establishment and amplification of Mendels' law, that law deals only with the interplay of allelomorphs, with the combinations and permutations of positive and negative unit properties possessed by the species. It only establishes the extent of variation possible *within the boundaries of the species*. But no amount of interplay of properties already possessed by the species will result in the production of individuals which are outside the species.

Sir Ray Lankester recently laid down that the one fallacy in all Lamarckian doctrine was that adopted by Herbert Spencer, namely, what he termed "direct adaptation." There is really no such thing. The supposed mysterious property of direct adaptation is always due to survival by selection of organisms which varied in many directions.

Now if there be one fact that is constantly being impressed upon the student of immunity and the workers in pathogenic bacteriology, it is that direct adaptation, i.e., specific modification in response to specific alteration in environment (within limits which he would lay down) is one of the basal phenomena of living matter. It seems useful, therefore, to marshal in order the data bearing upon these matters as they present themselves to those engaged in medical research.

Problems of this nature are *a priori* most likely to be solved by experiments upon the very simplest, and again upon the most complex forms of life. For problems of adaptation and heredity the bacteria possesses the supreme advantages of rapid reproduction coupled (according to our present knowledge, or want of knowledge) with a complete absence of the disturbing influence of sex and conjugation. Certain biologists are unwilling to regard the products of asexual binary division as true generations. One very distinguished biologist had said that a long cultivation of a bacterial growth is "one continuous individual." This is an impossible position. The very idea of individual connotes independent, or potentially independent, existence. We might with equal logic, basing ourselves on the continuity of the germplasm, declare that all living beings constitute one continuous individual.

It is absurd to expand the Batesonian hypothesis and imagine that whenever man became man he acquired the germs of all bodily ills, and that the purely human ailments were already there. Some diseases, like tuberculosis, have been with us from the remotest historical times, and even from prehistorical, as witness the late Sir Armaud Ruffer's studies in palæopathology upon mummies of early dynasties, and the recognition of caries and pyorrhœa in permian fishes and tertiary three-toed horses. This is only to be expected. The bacteria are among the earliest of all forms of life. Drew, from his studies of the calcareous ooze of the Florida lagoons, showed that a dentrifying bacillus caused the deposition of chalk out of sea-water. Walcott has discovered Cyanophyceæ and possible micrococci in the oldest of all sedimentary rocks, the Algonkian.

But this does not mean that all orders of pathogenic bacteria and microbes have always been with us. Zymotic phenomena must run parallel with geological. The vast majority of fossils are remains of species and genera which have passed away, but certain species, and, indeed, certain genera, have existed unchanged through countless ages to the present day. The brachiopod Lingula of the Cambrian rocks is to be found to-day living buried in the sand between the tide-marks in the Tropics. The pearly Nautilus, Limulus, Ceratodus and Anaspides have remained apparently unaltered for extraordinarily long periods of geological time.

The same would seem to be true with respect to zymotic diseases and their causative agents. Many of the plagues and epidemics mentioned by early writers are unrecognizable to-day. The tritest example of a disease which has come and gone is the malignant "sweating sickness," which, first noted in 1485, was last heard of in 1551. As regards diseases still with us, whatever view be taken regarding the origin of syphilis, it is certain that this was unknown in Egypt and in Rome at the time of Galen. Diphtheria and cholera, both with absolutely characteristic symptoms, were unknown in Europe until the beginning of the nineteenth century. Even if these two diseases had been locally endemic for long periods in some districts, we may come to the same conclusion as is reached in many cases of the geographical distribution of animals and plants. When a form introduced into a continental area rapidly spreads over that area, its previous absence is to be explained as due to the fact that the form in question originated at some period after the separation of the different continental areas. In this very war two new diseases, hitherto unknown, have made their appearance—trench fever and trench shin.

How, then, can we picture to ourselves the evolution of an infectious disease? In the first place, it is to be noted that pathogenic microbes are singularly diverse in their affiliations—there is scarcely a genus of micro-organism but has its representative or representatives among the pathogenic organisms—moulds, yeasts, fission-fungi, spirochætes, filterable viruses and chlamydozoa, amœbæ, flagellate and ciliate protozoa. *Every pathogenic microbe has closely related species differing from it in little beyond that the one is virulent, the other not.*

Next, the allied species are found suggestively growing in the cavities or on the surfaces of the body in the same habitat as the virulent forms, or, again, in the water and foodstuffs taken by the animal. This leads to the conclusion that pathogenic microbes at some period or periods have originated from forms saprophytic on the body surfaces, or in the foodstuffs, that they have originated by adaptation of these forms to growth, not on, but within, the tissues.

We possess abundant examples of experimental adaptation of bacteria to new foodstuffs, to foreign sugars, glucosides, fats, etc. from Pasteur onwards. The observations of Penfold, Twort, Massini and others upon the accustomance of bacteria to new sugars and their acquirement of the power of fermenting the same may be mentioned. The bacilli taking on these new powers were not mutants, the outcome of chance variation, but the acquired new property was definitely the result of a particular environment. Major F. B. Bowman has prepared a simple experiment which demonstrates that not some, but all, the members of a culture of bacilli, subjected to the same environment in a fluid medium of growth containing a foreign glucoside—isodulcite—acquire the new property.

Here, then, contrary to Bateson, we have evidence of positive acquirements from without, and, contrary to the Lankesterian dogma, we can so arrange our experiment as to obtain, not evidence of variation in many directions, but evidence that organisms placed in a given environment all vary in one identical direction with clockwork regularity.

If this be true regarding other properties, it must be true regarding the acquirement of virulence. As a matter of fact, Thiele and Embleton, at University College, had experimentally taken a harmless saprophytic form, the *B. mycoides*, accustomed it gradually to grow at the temperature of the body, and then, employing the dead bacilli to induce anaphylaxis and increased susceptibility, had, upon making a second injection, succeeded in obtaining the active growth of the bacilli in the tissues of the guinea-pig—and with this found that the bacillus was now virulent, killing other animals when injected into them. With these examples for consideration, is it possible for medical men not to believe in direct adaptation?

It is in respect to these new acquirements in the higher animals that we obtain the deepest insight into the processes involved, and that through the abundant, not to say overwhelming, studies of the last thirty years upon immunity. Yet although every man and woman of the day discusses familiarly matters such as typhoid inoculation, diphtheria antitoxin, and tuberculin, not a single general biologist has dwelt seriously upon the significance of these studies. *Immunisation is direct adaptation.* Take the familiar examples (to medical men) of immunisation to the phytotoxins, abrin, the active principle of the jequirity bean, and ricin, of the castor oil plant. The rabbit and guinea-pig have never come across these in nature. They are, in fact, intensely poisonous. One gram of ricin is adequate to kill 1,500,000 guinea-pigs. Feed these small animals with minute and progressively increasing doses, and eventually they can be given 100 times the fatal dose. And now 1 c.c. of the blood serum of the immunised animal will destroy ten, one hundred or one thousand times the fatal dose, according to the grade of immunity induced. Clearly, the blood serum now contains antitoxic substances, bodies which combine with the toxin, rendering it inert and harmless. The antitoxin has been elaborated and excreted into the blood by certain cells of the animal, and once these cells have acquired the property of elaborating an antiricin, they continue to produce it for weeks and months. Here is the acquirement of a new property—the acquirement is something positive, something added; there can be no alternative hypothesis of loss of inhibitory factors. Nor is it a chance variation: the power can be produced in any mouse or rabbit or guinea-pig with absolute certainty. Nor is it a matter of the survival of the fittest. The case of diphtheria and tetanus toxins, and the production of antitoxins against these toxins is absolutely parallel. The tissues can be educated to elaborate, and elaborate in excess, a body substance which neutralizes the toxin, and, once started, they continue for weeks and months to elaborate the antitoxin. It has been shown that it is the cells that take up and fix the toxins which elaborate the antitoxins. It is, however, only a minority of the pathogenic bacteria that form and excrete ectotoxins, poisons which are discharged into the fluid of growth; the majority do not excrete toxic substance. Nevertheless the body can be immunised against these also, though here the immunity is of a different order. It is bacteriolytic—a process of digestion. The fluids of the body gain the power of dissolving and digesting these bacteria. That power has been acquired by the millions of soldiers subjected to anti-typhoid inoculations.

This fact of the acquirement of a power on the part of the body fluids to digest the bacteria can easily be demonstrated. Every student of medicine has heard of Pfeiffer's reaction, in which the peritoneal fluid of a guinea-pig given progressive injections of the typhoid bacillus or the cholera spirillum, instead of forming a favorable culture medium for these bacteria now causes a rapid swelling up and dissolution, so that they melt away like sugar in water. Clearly, in the process of immunisation certain tissues of the body have gained the property of elaborating ferments which digest and dissolve the bacterial bodies, so that now, with little or no general reaction, the animal withstands many times the fatal dose of these pathogenic organisms. And this reaction is in general narrowly specific, so that it is employed to distinguish, for example, between closely related species of spirilla.

Nor is this necessarily merely a temporary acquirement on the part of the individual. For months after a man has been given one or two doses of dead typhoid bacilli his blood serum has a different physical constitution, or, as we are accustomed to term it, contains specific "anti-bodies"—agglutinins. The whole British Army now agglutinates the specific bacilli of typhoid, paratyphoid A and paratyphoid B, and that for a year or more after inoculation of the soldiers. Smallpox, naturally acquired, usually confers a life-long immunity. We here observe the working of a law which, if recognized, has not been dealt with adequately by biologists. In 1906 Wiegert, the Frankfort pathologist, laid down the law of inertia—the law that once a cell is stimulated to perform a certain act, it continues to perform that act for some

[1] Abstracts of four Croonian Lectures delivered at the Royal College of Physicians on June 14, 19, 21, and 26, by Prof. J. G. Adami, F.R.S., Temporary Lieut.-Colonel C.A.M.C.

[2] Sir E. Ray Lankester has in the *British Medical Journal* taken exception to the use of this word; the author agrees that "vigorously" better expresses his meaning.

time after the stimulus has ceased to be in operation. Here is something beyond mere inertia; the functional activity once started, at least in the order of events under consideration, continues too long to be comparable with physical momentum; rather there appears to be the setting in motion of a cyclic process of intercellular reactions and counter-reactions, the one starting the other. It is preferable, therefore, to employ a non-committal term, and to speak of the "law of habit." Of this law numerous examples may be given, both from among the bacteria and morbid states in man.

Next, to advance further, evidence obtained from medical research shows that acquirements, whether of defect or excess, are capable of being passed on to the next generation. There is abundant evidence of this in the case of the bacteria, and here the longer the environment has acted on a given species of microbe, the longer the microbe retains the impressed property, but as he could not state dogmatically that there is any biochemical property that is specifically fixed in these lower forms, still less could he regard any acquirement as being permanently fixed.

With regard to higher animals, difficulties are introduced by intra-uterine existence, so that the only clear cases to be considered are those in which the male parent alone has been subjected to the noxious or other influence. If the lymph contains soluble toxic substances, it is evident that the germ cells are not precluded from absorbing them, and, like the other tissue cells, from being influenced by them. There are many examples, clinical and experimental, of the effects upon the male germ cells of lead, nitrate of mercury, tuberculin, abrin, etc. The most conclusive observations are those of Prof. Stockard, of Cornell Medical College, New York, in which, by subjecting male guinea-pigs for some little period to the fumes of alcohol, he found not merely that the offspring were stunted and enfeebled, but that by crossing unrelated offspring of alcoholized fathers, which themselves had not been subjected to alcohol, the progeny of the third generation showed more extreme conditions of defect than did their parents. The importance of these observations upon the understanding of human family histories and inherited neuroses, etc., was very great.

It had been shown that the studies upon pathogenic bacteria and upon immunity prove conclusively the existence of direct adaptation of a definite order, both in the lowest and in the highest forms of life. It is along these lines that medical research is surely leading us.

Believing that workers in medicine are in the right, where is it that the other biologists have gone wrong? The latter, from the morphological trend of their studies, have perforce conjured up separate individual particles or structures, each the bearer of an individual property or group of properties. Their conceptions have perforce been in the terms of specific atomies. In his pangenesis hypothesis Charles Darwin evolved such a conception, and in his great sanity cast it aside. Weismann rioted in such, with his ids, idants and determinants, all figments of the imagination. The same tendency is shown and carried forward in full vigor by the modern Mendelians.

Suppose we start, instead, from known facts and known phenomena, and upon these endeavor to build up our idea of the nature of the germ cell and of the organic basis of heredity. First, as to the constitution of living matter. We know that whatever form of life we investigate, animal or plant, mammoth or microbe, whatever form we analyse or whatever tissue—leaving out of account water and certain vehicular salts to which no specific vital functions can be attributed—just one order of highly complex compounds is common to and to be isolated from all, and these are the proteins. This universal presence in itself indicates that they are intimately associated with vital functions. When isolated chemically they are inert; in other words, living matter contains proteidogenous, rather than proteid substances. Much attention has to be given to the study of the chemistry of the proteins in relationship to metabolism: the huge size of the protein molecule, close to the limits of visibility under the highest power of the microscope; its great molecular weight; the impossibility of gaining identical analyses of two samples of the same protein, even if, like hæmoglobin, crystallisable; the structure of these molecules; their dissociation into smaller complexes, the peptones; their further dissociation into amino-acids; the synthesis of the polypeptids. The protein molecules may, therefore, be represented as a ring or chain of linked peptone molecules, each having its ring of glycocoll nuclei with swinging side-chains. In the much simpler bodies with which the organic chemist is in the main concerned, bodies like the carbohydrates or the benzol derivatives, we know how the transfer of a given radicle from the *alpha* to the *delta* position, for example, upon a ring may bring about a profound change in the chemical and physical properties of the compound. When two carbon atoms are united together there are, or may be, six free affinities, and when these are satisfied by six different monovalent groups, twelve different isomeric arrangements are possible. What must be the possibilities in a protein like hæmoglobin, with 700 and more carbon atoms in the complex, and hæmoglobin is simple compared with the nucleoproteins.

If the biophores, or molecules of living matter, be at least proteidogenous, obviously it is not necessary to demand a separate determinant, a separate molecule for each specific property; it is simpler to regard properties inherent in the biophores as an expression of the constitution of the same, of the mode of linkage of the various nuclei, their number, and the nature of their side-chains. This conception is within the bounds of physical possibility; Weismann's ids and idants certainly are not.

Accepting this conception of the chemical constitution of the essential living matter as a working hypothesis, we know that in conjugation the one constituent of the germ cells contributed in an approximately equal portion by both parents to the zygote, or fertilized ovum, is the nuclear chromatin, and as heritage of properties may come equally from either parent, in the nuclear chromatin must reside the main heritable and character determining material. The conclusion is inevitable that the essential biophoric molecules are conveyed in the nuclear chromatin. The cell-wall, the cytoplasm, and the nuclear membrane are all conservative agents, tending to preserve the biophores from sudden change from without, but while conservative, this system is exposed to constant change, particularly in the more active tissue cells. The system is not inert, but is constantly reacting with the external medium in which the cell finds itself. The semi-permeable cell membrane, while preventing the entrance of some substance, freely permits the entrance of others, whether directly or after a preliminary dissociation into smaller molecules by the action of extracellular enzymes. Once foodstuffs are taken into the cytoplasm they are, if necessary, broken down into yet simpler molecules by intracellular enzyme action. Foodstuffs are not utilized by the cell as such, but only after dissociation and disintegration, and then either by oxidation to supply energy or, on the other hand, to be built up in growth.

This matter of growth is wholly neglected by the other biologists. They speak of inorganic bodies (crystals) growing by agglutination, organic bodies by intussusception. "Intussusception," "imbibition," "intercalation," and "interpenetration," are all inane terms; they cannot possibly explain how two molecules of living matter appear where there was but one before, two grains of wheat where but one was put into the ground. Growth is one of the great underlying phenomena of living matter, and zoologists and botanists have in a simple Topsy-like manner been satisfied that the phenomena occurs—and have left it at that. Increase in the amount of living matter means multiplication of the molecules of living matter, and this multiplication can only take place after the manner of the growth of a crystal, by ions arranging themselves into radicles, and radicles arranging themselves in a particular order, until in orderly sequence the necessary radicles become built up, identical in arrangement with the pre-existing molecule, in association with which the group has become developed. This conception is materially aided by the recognition that crystallization does not of necessity demand the production of rigid rectilinear figures. Lehmann in 1904 first directed attention to the existence of "fluid crystals"; in 1906 Adami and Aschoff pointed out that these fluid crystals are frequent in the animal organism. As D'Arcy Thompson remarks, "the phenomenon of liquid crystallization does not destroy the distinction between crystalline and colloid forms, but gives added unity and continuity to the whole series of phenomena."

Weismann's doctrine of the continuity of the germplasm is erroneous; it is not the germplasm which is eternal; merely there is a potential continuity of molecular arrangement and constitution. The functional and vegetative activities of the organism and the cell, along with the essential nature of metabolism and enzyme action, emphasize that these matters of adaptation and evolution have to be approached from the aspect of function and the dynamics of living matter, rather than from the point of view of cell statics. "Function precedes structure," and the study of cell function must afford the key.

As regards the acquirement of the new power of digesting and utilizing a foreign protein, it is seen from what has been said that these proteins are complexes of amino-acids; and the number of the individual amino-acids is limited. Proteolytic enzymes, already in existence, whether intra- or extra-cellular, do not attack the foreign protein as a whole, but must be regarded as dissociating certain every-day amino-acids from the complex. But doing this, to take the simplest case, the relative number of molecules of the different amino-acids presented to the cell may come to differ from the normal, or, again, the simpler complexes due to the breaking down of the foreign proteins may not be identical in constitution with those which the cell and its biophores had been accustomed to utilize in growth. In either case the constitution of the biophores may become altered as they are built up. Where enzyme-like bodies, such as the toxins and phytotoxins, become introduced into the cytoplasm, their toxic function must be regarded as due to their power of dissociating the living molecules, by detaching certain radicles. If the toxic molecules be not present in too great a number, time is given for the living molecules to attract and build up again the lost radicles, and by the law of habit, if this process be constantly repeated, particular radicles are now to be built up in excess of the needs of the cell, and, undergoing discharge, become the antitoxin bodies of the blood and body fluids.

That this conception of the mechanism of immunity and progressive adaptation is substantially correct was strongly supported by the long-continued and admirable studies of Prof. V. C. Vaughan, of the University of Michigan, and the later work of Abderhalden, of Berlin, and his pupils.

The prevalent conception of the Mendelians that the parental properties remain segregated in the germ cells is open to attack. In the zygote the fertilized germ cell, and in all the tissue cells derived therefrom, it is inconceivable that two orders of biophores, or active living molecules, can exist floating in a common nuclear sap, undergoing growth, building up side-chains and radicles, discharging certain of these, or undergoing dissociation from time to time, without the two reacting upon each other, and without a certain amount of interchange, without the one having a greater affinity for side-chains elaborated by the other and building these into its system. There must be this interaction, and at a slower rate, due to their more latent state, this same interchange must take place in the germ cells. Along these lines it is still possible to interpret the facts of Mendelism, and, indeed, interpret not a few phenomena which by the hypothesis of determinants fail to obtain explanation.

Briefly, each species must be regarded as having for its essential living matter a distinct organic compound, a compound as distinct as any inorganic salt, but differing from that simpler salt in that whereas the central ring, or chain, is to be regarded as having a relatively fixed constitution, the radicles composing that ring or chain are to be regarded as capable of attracting and then reproducing a series of side-chains which may vary in constitution, so that within the species there may be various strains, just as we may speak of various strains of crystalline hæmoglobin obtained from different samples of human blood.

It is possible to replace an impossible hypothesis based upon suppositious independent and transposable determinants by one based upon our present knowledge of the composition and properties of the main and outstanding constituent of living matter—the proteins. To one who regards life, not from the morphological point of view, in terms of form, but from the physiological, in terms of function, who regards life as a moving equilibrium, who regards it as in essence "a state of persistent and incomplete recurrent satisfaction and dissatisfaction of certain proteidogenous molecules," and metabolism as the primary and basal characteristic of living matter, for such a one Prof. Batesons' stumbling-block does not exist.

The hypothesis of a backward evolution by the progressive removal of inhibitory factors, like the baseless fabric of a vision, fades into nothingness once it is confronted by the proof that direct positive acquirements can be brought about experimentally. It enters into the limbo of the past as an example of the Spencerian tragedy—that of a deduction destroyed by a fact.

The Uneconomical Horse

It is said that the efficiency of the horse is but 2 per cent, while he consumes annually the product of 5 acres.

Fig. 1.—Magen's Bay, St. Thomas

Fig. 2.—Clearing undergrowth by fire

Fig. 3.—Method of making excavations

Archeological Investigations in the Virgin Islands

To Solve the Riddle of the Origin of Their Aborigines

By Theodoor De Booy

THE Museum of the American Indian-Heye Foundation—of New York City was the first scientific institution to send an expedition to the Danish West Indies when negotiations between the United States and the Danish Government regarding the transfer of the Danish West Indies to the American flag had progressed so far that the sale of the islands had become a certainty. The object of this expedition was to collect such pre-Columbian specimens as might be found upon the islands and to gather data which would throw light upon their aboriginal population. The officials of the Danish Government stationed on the islands were more than helpful and put every facility at the expedition's disposal to further its undertaking and sincere thanks for permission to excavate on his Magen's Bay Estate are due to Dr. P. Mortensen of Charlotte Amalia, St. Thomas.

In order to understand the methods by which aboriginal village sites are generally located in the West Indies, it may be well to preface this article by stating that after arriving on whatever island he wishes to investigate, the archeologist begins by asking the native agriculturists whether they have ever noticed any large quantities of shells on the land they possess. The reason for this query is that the early inhabitants of the West Indies made shell-food one of the principal articles of their diet; that, after eating the molluscs, they naturally threw the empty shells in heaps near their dwellings and as these shells cannot be destroyed by the climate, they remain today as mute evidence of the location of the pre-historic village. In consequence, if the visiting archeologist finds a native who assures him of large shell deposits on his little farm, and the report proves to be true, there is absolute certainty that the finding of Indian artifacts remains but a matter of excavating. The reverse of this rule has, of course, no value; that is to say, if no native can be found who knows of shell-deposits, the archeologist need not necessarily think that there was no Indian occupation. In the first place, the natives may have been unobservant and it has frequently been found that while shells were actually found in large quantities on their fields, they had become so used to the presence of these that they had forgotten the fact entirely. In the second place, the diluvial deposits formed in the centuries after the departure of the Indians may be so thick that the shells are covered by two feet or more of humus and in consequence are never seen by the agriculturist. This proved to be the case on the island of St. Thomas, the westernmost of the three principal islands of the former Danish West Indies. All inquiries as to Indian specimens or the presence of shell-heaps proved to be in vain and while the inhabitants were well informed as to the location of pre-historic village sites on the island of St. Croix, they were equally certain that nothing could be found on St. Thomas. But one person on St. Thomas knew of an actual shell-heap and an investigation of this heap was discouraging as it contained thousands of shells, but not even the smallest Indian specimen to indicate that the shell-heap had been made by the aborigines of the island. Of this shell-heap later mention will be made.

There being, therefore, nothing to go by in the way of surface indications, all that could be done was to make a systematic survey of the island. In order to do this, one has to be acquainted with the manner in which the aborigines lived—an acquaintance which cannot be learned from books but which one can only pick up after years of field experience and innumerable mistakes—and to imagine one's self in the Indians' place. It will generally be found that, on the smaller of the West India islands, the village sites are located in close proximity to sheltered bays and inlets where the aborigines found a sloping beach for the hauling-up of their canoes, and where the surrounding waters assured them of an unfailing supply of fish and shellfood. Add to this the presence of fresh water, found in either ponds, springs or a rivulet, and when one has discovered a bay that possesses all these attributes, the chances are favorable for the finding of the prehistoric abodes. This then was the method used to locate a place to excavate on St. Thomas; and after ineffectual surveys of a number of the bays that are found on the island, success was at last met with on the shores of Magen's Bay, the largest inlet on the north coast. *Figure 1.*

Fig. 9.—Swallow stick found at Magen's Bay

There can be no doubt that Magen's Bay was an ideal location for the Indian village. In the first place, with the exception of northerly storms, the bay is well-sheltered and its sloping sandy beach made the disposal of canoes an easy matter. Not only this, but the presence of the giant *ceiba* (cottonwood) trees on the hillslopes nearby, assured the Indians of an unfailing supply of material for their dug-out canoes. The waters of Magen's Bay abound not only with shell-food, but are a veritable mine of fish of all varieties. Birds and bird-eggs and the now extinct *Isolobodon portoricensis* (a mammal about the size of a rabbit) helped to fill the Indian's wants for animal-food, and the hills and valleys to the back of Magen's Bay, undoubtedly, served admirably for the cultivation of cassava, yucca and the hundred and one fruits that make life delightful in the West Indies. Also, springs and pools furnished the Indian household with the required water. It will be seen, therefore, that this particular spot on St. Thomas was well adapted for the Indian settlement.

The actual location of the Magen's Bay village-site was a difficult matter, as the dense brush with which the shores of the bay were covered made the search for low mounds almost impossible. Minute pottery fragments were found on the ground and served to convince that the actual finding of the deposit was but a matter of time and patience. These pottery fragments were widely scattered and as the numerous watercourses which exist in the Magen's Bay valley during the rainy season undoubtedly were responsible for their distribution, they did not serve as a guide to the spot where to commence excavations. Finally, an uprooted tree solved the puzzle. An examination of the cavity left by the roots of a large "turpentine-tree" which had been overturned by the destructive hurricane of October 9, 1916, brought many shells and several large potsherds to light. It was then that the realization came that the entire Magen's Bay valley had been covered by a two foot deep deposit of diluvium since the days of the aborigines, and that in consequence there was no use in looking for hummocks or mounds of prehistoric origin as the surface of the valley had been covered and made almost level by the decomposing humus.

Owing to the dense brush it was found necessary to first set a gang of men to work to chop down the smaller trees and brushes and to set fire to these. *Figure 2.* After this was done it became possible to observe the presence of a ten-foot high mound, the presence of which had previously been unnoticed, as it merged with the slope of a small hill, against which it lay. Excavations were started in this mound with a considerable number of workmen, and a trench was commenced in the mound, which had a semi-lunar shape, a width of about seventy-five feet and a breadth of thirty feet. The eastern slope of this mound was limited by the slope of the hill and the western slope by a little path which ran through the undergrowth. The mound, as has been previously stated, was almost

Fig. 4.—The shell heap at Magan's Bay

indiscernible as it was impossible to make out where the mound stopped and the hill-slope commenced until after the excavations had been made. In fact, had it not been for the accidental observance of the fallen tree, it might have been a long time before the expedition would have started excavations in this particular spot, from the fact that the appearance of the ground made one think more of a hill-slope than of a pre-Columbian deposit.

A trench some thirty feet wide was made in an easterly direction, directly towards the slope of the hill. *Figure 3.* It is generally found best in West India work of this kind to first make a hole some six feet wide over the entire length of the trench—in this instance thirty feet—and to dig down to the original bottom which existed before the forming of the Indian mound. While making this hole, there is but scant possibility of preventing the artifacts that may be found from breakage, as the working with a pick and shovel in a perpendicular direction is not conducive of the preservation of specimens of a friable nature. Once this hole is made, however, the workmen proceed in a straight direction by undermining the wall of the trench in the lowest part of the excavation. It will then be found that the earth, shells and ashes above will fall down by their own weight, and it is an easy matter to pick the Indian specimens from the debris. The bottom of the excavation is always kept as clean as possible, so that the smaller artifacts such as stone beads, amulets, etc., can be the more easily discovered.

Volumes could be written about the destructive roots that were found in the Magen's Bay deposits. The Magen's Bay valley is especially rich in genip trees (*Melecocca bijuga*), and the roots of this tree rank easily as the most penetrating and destructive root found in the West Indies. While practically nothing but broken pottery was thrown on the refuse-heaps by the Indians (a refuse-heap or kitchen-midden being nothing but a place where the aborigines threw their empty shells, their broken stone implements, such pottery as happened to break during usage, the ashes of their fires, etc.), the fragments so thrown would not necessarily be small; the root of the genip tree, with penetrating force, succeeds in breaking these sherds up into small pieces until finally but very few large fragments remain. In addition to causing this damage, and making the work of the archeologist harder, the trees themselves offer a serious danger to the workmen engaged in excavating. *Figure 4.* During the digging of the trench, as has been stated, the men undermine the lower part of the wall and allow the earth of the upper layers to fall down. This process results in the forming of small caves, the roofs of which consist of the sod and the diluvial deposit with which the mound is covered. Frequently the stump of a genip tree with its tough roots, weighing perhaps a couple of thousand pounds or more, would be found in this surface layer, and if the men omitted to watch this stump it was not an unusual occurrence to have the entire mass break down the roof of the little cave and fall some six feet or so to the bottom of the excavation, carrying a great deal of the surrounding earth with it. Had any of the men been working in the wall of the trench when one of these falls took place, it might have been a decidedly hard matter to remove the tree-trunk and the earth before the laborer had been suffocated.

In order to obtain a clearer idea of an Indian kitchen-midden or refuse-heap, it will be necessary to examine the accompanying photograph which shows a cross-section of the Magen's Bay deposit on St. Thomas.

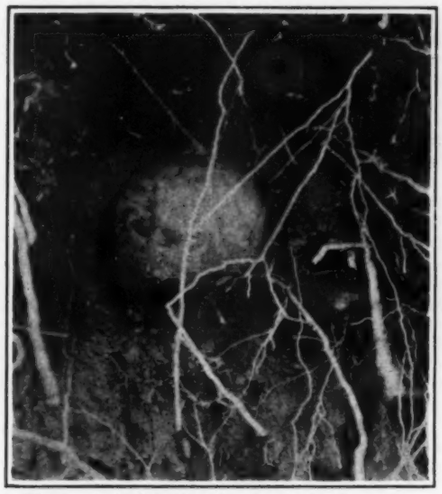


Fig. 8.—A skull in situ, showing destructive roots

Figure 5. Beginning on the surface, at the white line, one first finds a deposit of about two feet thick of diluvium. It will be noted that this deposit is somewhat lighter in color than the lower ones. This diluvium or humus was formed in the course of centuries, *after* the Indians had abandoned the site, by the decomposition of the leaves, branches, roots, etc., that fell upon the surface of the mound. The grasses

Fig. 5.—Section of aboriginal deposit, Magen's Bay

Fig. 6.—Pottery vessel in situ

Fig. 7.—Washing and drying potsherds

growing upon the surface form a kind of sod and what with this, and the entertwining of the roots of the trees and bushes, the first layer generally is quite tough and hard to dig in. The second white line from the top indicates the commencement of the Indian deposit. From the second line to the third line is about 2½ feet, and from the third line to the fourth about the same distance. The two dark layers between these lines were formed during the aboriginal occupancy.

These deposits consist of a mixture of humus, ashes, charcoal, artifacts, shells and the bones of such animals as were eaten by the Indians. An occasional burial was found in the lower of the two dark layers, but fully eighty per cent. of the burials were found in the sea sand, directly under the fourth white line shown in the illustration. Under these dark layers is found the original surface as it existed before the coming of the Indians. In this instance, the surface is of sea sand, which urges that the shores of Magen's Bay were some hundred feet further inland in prehistoric days, and that the Indians had their village-site on the beach, and almost on the very edge of the bay. It was in this white sea sand that the majority of the burials were found.

As can be seen, the roots of the trees found on the site penetrated not only the second and the third layer, but were even found in the sand underneath.

While the majority of the artifacts found in the two layers directly under the diluvial deposit were the fragments of broken vessels, entire vessels were occasionally met with. Frequently, in fact in the majority of cases, these vessels accompanied burials, and in consequence had been buried entire, probably filled with foodstuffs which were to serve the departed for his journey to the Great Beyond. But sometimes a vessel would turn up in the Magen's Bay deposits which was unaccompanied by a burial, and which consequently must have been either discarded by its Indian owner, owing to a flaw in the ware, or else have accidentally been buried under a mound of shells, *Figure 6.* The utmost care had to be used in removing a vessel from the trench, and the digging-out of one of these was generally only undertaken by the leader of the expedition. It was impossible to tell whether or not the roots had developed cracks during the centuries that the fragile specimen had awaited the coming of the archeologist, and it was no infrequent occurrence to have a bowl fall into numerous pieces on being lifted from its matrix. These pieces then were carefully packed together, and on arrival at the Museum in New York are cleaned, glued together and mended in such a manner that the breaks become practically invisible.

Not only are the pieces of the entire vessels saved, but the hundreds of potsherds found in the second and third layers of the deposits are "matched" as far as it is possible to do so and afterwards mended. At times surprising results are attained by doing this. It frequently occurs that one will find the fragment of a bowl in one place and a piece of the same bowl some fifteen feet away. The explanation of this is that when an Indian woman broke one of her cooking pots, she would throw the pieces on the refuse heap back of her hut. One piece might fall in one direction and another piece in some different way. After years of practise the archeologist develops a good memory for the fragments he excavates; he mentally classifies these by their color and the thickness of the ware, and by doing this in many instances is able to reconstruct a bowl out of perhaps twenty different sherds. The best method is always to do

this "matching" in the field first, and then to go over the material again after it reaches the Museum. On the St. Thomas expedition, the sherds were washed as they came out of the kitchen-midden, and then placed on a large tarpaulin to dry, *Figure* 7. Such piece of bowls as belonged together were then marked with pencil scratches, and a close examination of the accompanying illustration will reveal, among other sherds, the half of one bowl, lying on the tarpaulin, which large fragment contains a number of sherds belonging to the same vessel.

Not only potsherds are found in the second and the third layers. Broken stone implements, stone beads, shell- and bone-pendants, bone awls, etc., are among the hundreds of objects that enable the archeologist to gather his data on the aboriginal inhabitants. Perhaps the most important item of all is the examination and identification of the shells and of the animal bones that are found in these layers. All animals bones are preserved and are sent on to the United States National Museum in Washington by the Museum of the American Indian-Heye Foundation, there to be identified by the various experts. So are the shells saved and sent on for identification. After this is done, one can state the exact components of the animal diet of the aboriginee. It was found in St. Thomas that the principal shell-food of the Indians consisted of the *Arca occidentalis* and the *Strombus gigas* (conch), and that, in all, 26 varieties of shells were met with. Of mammals, only the bones of the *Isolobodon portoricensis* were found, a small mammal which is now extinct in the West Indies. Of birds, the Indians seem to have eaten the Tree Duck (*Dendrocygna* spec.), the Frigate Bird (*Fregata* spec.), the Night Heron (*Nyctanassa* spec.), the Petrel (*Of strelata*), the Gannet (*Sula* spec.), and a species of flightless bird hitherto unknown which has been named the Island-runner (*Nesotrochis debooyi*), by the ornithological expert of the Biological Survey. Of reptiles, the bones of the Green Turtle (*Chelonia mydas*) and of the Iguana (*Iguana* spec.), were encountered. Of fishes, the Indians seem to have eaten not less than eight varieties, with possibly many others of which no bones were found in the kitchen-midden. Also, many claws of the common, light-colored land-crab (*Cardisoma guanhumi*), proved that the aborigines did not despise this animal in their dietary. It will be seen, therefore, that the shells and the bones found in the kitchen-middens not only are important as being evidence of the food of the Indians, but frequently are of the highest zoological interest where they belong to animals which are now extinct.

It was in the original surface of sea sand some five feet below the surface of their mound that the Indians seem to have deposited their dead. The practise of burials in the floors of dwellings was not uncommon on many of the West India islands, as has been proven by the researches of other investigators. Of the nine burials that were found on the Magen's Bay site, seven were found of adults and two of children. Six of the burials were accompanied by mortuary vessels, but outside of these vessels no other objects were found with the dead. The vessels were of the plainest construction, without decoration, and were a great contrast to the elaborately decorated vessels that are found on the islands to the westward, Porto Rico and Santo Domingo. Judging from these, and from the other objects found in the deposits, the aborigines of St. Thomas decidedly were not of Arawak stock. Neither did their artifacts in any way resemble the Carib culture from the islands to the south, Guadeloupe, St. Lucia, St. Vincent and Grenada. In consequence it will be impossible to classify the tribes inhabiting St. Thomas, St. John and St. Croix until additional researches have been made on the other British islands of the same group, Tortola, Virgin Gordo and Anegada. The skeletal remains found were in very bad condition, which was not only due to the destructive roots but also to the moisture contained in the sea sand in which they were found, *Figure 8*. But very few of the skulls could be saved, and practically none of the other bones. As the skulls, owing to aboriginal practise of head-flattening, are of but little value to the anthropologist from the fact that the flattening precludes the making of systematic measurements, the riddle of the origin of this isolated tribe cannot be solved by anthropometrical means. That the tribe inhabiting St. Thomas either made piratical raids on its neighbors to the west, or else held commerce with them is proven, beyond a doubt, by some dozen typical Arawak specimens that were found in the kitchen-middens. These specimens differed in both art and technique from the thousands of other specimens found, and can be duplicated in almost any collection from Porto Rico or Santo Domingo. Perhaps the most interesting of these was a so-called "swallow-stick," *Figure IX*, made of the rib of the sea-cow. Three like specimens can be found in the collection from Santo Domingo in the United States National Museum in Washington, although the Washington specimens lack the inlay of mother-of-pearl which was used by the aboriginal artist to depict the eyes and mouth of the deity surmounting the stick. From the early Spanish historians we know that it was the custom of the native priests to introduce this stick in their throats in order to induce vomiting before worshipping their *zemis*, this being the native name for their deities. There can be no doubt that the St. Thomas specimen originated on either Porto Rico or Santo Domingo; whether the Indians of St. Thomas acquired it peacefully, or carried it off as a trophy after a successful raid on the peaceful Arawak tribes inhabiting the former island, can, of course, not be stated positively.

The only other aboriginal deposit found on St. Thomas was an extensive shell-heap at Krum Bay on the south coast of the island. As has been stated, no Indian specimens were found here, and the only feasible theory that would account for these shells would be that the Indians went from the north coast to Krum Bay to fish for these shells, and opened them on the spot and then carried the molluscs to their home at Magen's Bay. An extensive and thorough survey of the bays of the island convinced the expedition that no other village-sites could be found on St. Thomas.

Besides the St. Thomas survey, the expedition carried out an archeological survey of the islands of St. John and St. Croix. Beyond a number of rock carvings, no evidences were found of an aboriginal occupation of St. John, despite the fact that the entire island was gone over in the most careful manner. It is quite likely therefore that St. John was only used by the Indians from St. Thomas and St. Croix as a meeting place for ceremonial purposes, and that the rock carvings on St. John have been made during these ceremonies. St. Croix, on the other hand, must have sheltered an extensive prehistoric population, and over ten different village-sites have been discovered by various investigators. Extensive excavations were carried on at Salt River by the expedition, but as the specimens found, and the conditions under which they were found, were identical with those encountered on St. Thomas, a discussion of this part of the work would be out of place in this article. It is hoped that, at some future time, researches may be carried out on the other islands of the Virgin Island group in order to complete the collections from this region, and to attempt, by comparative studies, to solve the riddle of the origin of the aborigines of St. Thomas and St. Croix.

Remarks on Terms of Relationship[1]

Truman Michelson, Bureau of American Ethnology

Some years ago Kroeber[2] undertook to show that terms of relationship are linguistic and psychological phenomena. Recently Rivers[3] has attempted to overthrow this view, holding that they are sociological phenomena, and consequently that it is entirely possible to infer marriage customs and social organization from these terms. Lowie[4] to a certain extent followed Rivers but has not followed the latter's survival-theories, nor is it likely that many American ethnologists will do so.[5] The present writer[6] developed Kroeber's linguistic thesis from a different angle, and also made a new point, namely, that terms of relationship are likewise disseminative phenomena. Specific data from Algonquian tribes were given to establish these facts. Lowie,[7] some months later, but quite independently, arrived also at this second theoretic position but extended the principle more broadly than the present writer had done. It is not without interest to note that we both assume that Iroquoian and Siouan influence has played a part in Algonquian terms of relationship. Sapir[8] briefly touches upon the methodological considerations and concludes that thoroughly satisfactory results can not be secured without linguistic analysis of kinship terms; that existing nomenclature may be retained in the face of sociological developments requiring its modification; that the factors governing kinship nomenclature are very complicated. Goldenweiser,[9] in his review of Rivers' *History of Melanesian Society*, says, "A set of terms must always remain a feature of language and as such it is subject to those influences which control linguistic changes as well as to the peculiar spirit of a particular language or linguistic stock." The present writer[10] has shown what extraordinary types of marriage we should have to assume existed formerly among the Piegans, were we to believe that marriage customs might safely be inferred from terms of relationship; whereas such marriages are fundamentally repugnant to the Piegans, and their terms of relationship are new, not old. This is, of course, a concrete example of one of Sapir's points. Recently Swanton[11] quite similarly brings forward data from Creek and Chickasaw which prove the unsoundness of such inferences.

The above has been cited to show that American ethnologists generally have taken a united stand against Rivers' one-sided attitude. I reopen the case because none of us has given absolute proof that kinship terms are borrowed. In another place[12] I have tried to prove that Cree has borrowed certain terms from Ojibwa; and somewhat similarly that Peoria has been influenced by Sauk, etc. I think the reasoning given there is sound, and as near absolute proof as we can expect to have in the case of prehistoric linguistic borrowing; yet it is not absolute in so far as we have no Cree nor Ojibwa records transmitted to us historically, extending over several centuries, showing absolutely that such borrowing took place. The same is true regarding Peoria. English is a good language to draw on for illustrative material to prove such a point, for it has been transmitted historically for several centuries. Every Indo-European philologist knows that *sister* is Scandinavian in origin, and that *cousin*, *niece*, *nephew*, *aunt*, and *uncle* are Romance. Furthermore all our terms of *-in-law* are directly or indirectly due to the latter's influence; *grand-father* and *grandmother* are Romance in the first member of their compounds. Similarly Albanian *frat* "brother" is Romance in origin and is not a native word, as is shown by the phonetics. In the same way Hungarian *barát* "brother" is borrowed from Slavic, a case of borrowing across linguistic stocks. [Ojibwa *nimpapa*, *nimama* (Fort William) are other illustrations of kinship terms borrowed across linguistic stocks.] These facts, long known, are brought forward simply because they seem to have escaped the attention of ethnologists. Delbrück[13] was well aware of the fact that Indo-European terms of relationship are linguistic phenomena. Several terms that correspond phonetically occur in so many Indo-European languages that it is evident they belonged to the Indo-European parent language. The social organization of the peoples speaking the historical languages had nothing to do with it, as is shown by their diverse social organization. Nor can it be said that such German compounds as *Schwiegermutter*, *Schwieger-vater*, *Schwieger-tochter* have replaced the Old High German words by reason of a change in social organization. Similarly, the fact that the Slavic word for "father" (Old Bulgarian *otĭcĭ*) has a different termination than Greek arra, Gothic atta, is of linguistic significance, not sociological. These data support my contention referred to previously.[14]

Let us return once more to Rivers' position. On looking over the tables given at the end of volume 1 of Rivers' *History of Melanesian Society*, I am convinced that after all, he may have just as much a linguistic and disseminative problem as a sociological one. For example, the social organization of Mota, Banks Islands, and Eddystone, Solomon Islands, is entirely different; nevertheless the terms for *father*, *elder brother*, and *younger brother* are evidently the same. Furthermore, the distribution of slightly varying forms of the words *tama*, *tina*, (*father* and *mother* respectively) even across linguistic stock, points in the same direction. As I am not a specialist in Melanesian and Polynesian linguistics. I regret that I can not thresh this out to the end, and can only indicate a problem for others to solve.

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[1] Published with the permission of the Secretary of the Smithsonian Institution in the Journal of the Washington Academy of Sciences.

[2] Journ. Roy. Anthrop. Inst. Gr. Brit. and Irel., 39: 77-84. 1909.

[3] Kinship and Social Organization, 1914. The History of Melanesian Society, 1914.

[4] Proc. Nat. Acad. Sci., 1: 346-349. 1915. Amer. Anthrop. n. ser., 17: 223-239, 329-340, 588-591. 1915.

[5] Webster (Amer. Anthrop., n. ser., 17: 175-177. 1915) is an isolated exception.

[6] Proc. Nat. Acad. Sci., 2: 297. May, 1916.

[7] Holmes Anniversary Volume, 293. December, 1916.

[8] Amer. Anthrop., n. ser., 18: 327, footnote 1. 1916.

[9] Science, n. ser., 44: 826. 1916.

[10] Holmes Anniversary Volume, 353. 1916.

[11] Amer. Anthrop. n. ser., 18: 463. 1916 [1917].

[12] Proc. Nat. Acad. Sci., 2: 297. 1916.

[13] Die Verwandtschaftnamen. Leipsig. 1889. Also in Abh. phil.-hist. Klasse sächs. Ges. Wiss., 11: 379. 1889.

The Fumigation of Greenhouses*

The Effect on Plants, and Some Practical Suggestions

By G. E. Stone

THE use of various fumigants, especially of hydrocyanic gas, has become one of the most effective means of controlling various insects that are injurious to plants grown in greenhouse culture. Fumigation, however, very frequently results in more or less injury to the plants through the so-called "burning" of leaves and flowers, so that judgment and care must be exercised in the application of fumigants. A greenhouse may be fumigated at one time without injury to the plants, whereas the same dosage applied a few days later under apparently the same conditions will cause serious burning to the foliage. This often proves very perplexing to gardeners, and while the condition of the plants may appear the same and present the same relative degree of development in both instances, they are, it would seem, in a different condition. The cause of this, however, is not difficult of explanation. Plants, and especially various organs of a plant, are never under precisely the same conditions on any two consecutive days for the reason that they are not developed under the same environment. The different factors essential to plant growth are constantly varying in intensity and the optimum for any one, and more especially for the proper balance of them all, is scarcely maintained for any length of time.

The light, temperature, moisture and ventilation, etc., conditions are always variable, hence there occurs corresponding variation in the development of the organs. If absolutely identical conditions could be maintained, susceptibility to burning in one case would not be different from that in another, except in cases where variations associated with intricate life-cycle periods are involved for a plant or an organ. Any agency which affects the development of plants is likely to render them more or less susceptible to burning, and this is true of light and of soil moisture. Experiments have shown that when plants are grown under different light intensitites or in different percentages of soil moisture they become greatly modified in their structure, and such modifications in their development are very frequently associated with susceptibility or nonsusceptibility to burning.

In some experiments made in a greenhouse during the spring months under cloth screens where the relative light intensity to which the plants are subjected was controlled for different intensity, the susceptibility to burning from fumigating with hydrocyanic acid gas was greatest in the plants developed under poor light conditions and the amount of burning decreased proportionately as the light conditions improved. For this experiment five sets of cucumber plants were grown under different degrees of light intensity for a period of several days, but with all other conditions as near uniform as was possible. The following tabulation indicates the noteworthy differences in the growth of the different sets, when tested for susceptibility to burning by fumigation. The mensurements given are in centimeters.

	Series				
	1	2	3	4	5*N*
Relative light intensity (per cent.)	24.00	26.00	48.00	74.00	100.00
Average height of plants	22.30	23.30	17.30	19.00	15.50
Average diameter of stem	.44	.53	.53	.66	.78
Average length of internodes	17.30	7.00	5.00	4.30	4.00
Average length and width of leaves	70.50	90.30	62.60	80.20	85.50

The more nearly normal plants designated as 5*N* which are taken as 100 per cent were exposed to the regular light conditions which existed in the greenhouse, and which averaged about 18 per cent less intense than those outdoors. The other plants, series 1-4, were grown under cloth screens, their relative light intensity exposure being given under their respective numbers. It will be observed that very marked differences exist in the configuration of the plants due to the variation in light intensity.

There occurred a general decrease in burning from the use of hydrocyanic acid gas from No. 1, where the light intensity is represented by 24 per cent, up to 5*N*, where it is taken as 100. The leaves of the plants developed in the poorest light were burned badly and in some cases killed, whereas in 5*N* no burning occurred and in 2, 3 and 4 burning was proportionate to the light condition under which they developed.

In this experiment the larger and more vigorous plants, which were grown under the higher light intensities were more resistant to the burning effects of gas.

Definite results were also obtained with cucumber plants which were grown in soils with varying percentages of soil moisture ranging from 10 to 70 per cent of their total water-retaining capacity. The burning resulting from fumigation with hydrocyanic acid gas was the most marked in the plants grown in the soil containing the highest percentage of soil moisture and the burning decreased proportionately as the soil moisture was diminished. Here there were marked differences in the relative growth of the six sets of plants, as will be seen in the following tabulation.

In respect to nearly all average measurements there was increased size with increased water supply. After giving these sets the same exposure to gas it was found

	Series					
	1	2	3	4	5	6
Soil moisture per cent.	10.00	15.00	20.00	50.00	60 00	70.00
Average height	6.20	7.5	10.2	13.00	17.50	21.2
Average length of internodes	4.20	4.5	5.2	5.50	7.50	8.2
Average length of peticles	1.30	1.7	2 2	3.20	4.00	3.2
Average diameter of stem	.30	3	.4	.45	55	5
Average length and width of leaf	6.25	39.5	67.5	162 50	225.00	285.00

that in Series 1 and 2 there was no burning; Series 3 showed slight burning; while in Series 4, 5 and 6 the leaves were all burned. The burning was most marked in Series 6, which was grown in pots containing the largest amount of soil moisture.

These results show very clearly how different conditions with respect to such factors as light and moisture may very decidedly influence the susceptibility of a plant of such individual organs as leaves to the burning effects of gas used in fumigation.

In respect to the influences of light alone, it appears that the largest and most vigorous plants were most resistant; but in respect to moisture supply the smaller slow-growing plants that developed with the lower water supply were most resistant. Such results indicate clearly that the general conditions under which plants develop, or under which different organs such as leaves develop, are of decided influence in determining the susceptibility of the plant or the organ.

Another point of considerable importance is the particular degree of activity of a plant at the time of fumigation. The susceptibility of a particular plant varies according to its activity. For this reason gardeners and florists have for some time held the idea that fumigation must be done at certain times of the day in order not to cause burning, and most fumigation is done at night or early in the morning on the supposition that less burning is likely to occur to plants at such periods than during bright sunshine. The effect of sunlight and atmospheric moisture, etc., on the burning of tomatoes and cucumbers has been studied by investigators who have found that sunlight, cloudy weather, moonlight, drops of water remaining on the foliage, and too high a moisture content of the atmosphere, were all conducive to burning, and that less burning occurred on cloudy nights or on clear nights without moonlight. The best results were obtained by fumigating on clear starlight nights, with little or no moonlight, or on dry cloudy nights with a temperature ranging from 55° to 65° F.

The prevailing idea long held by most greenhouse managers, that less burning occurs at night from the various fumigants, is supported by recent investigations relative to injuries to vegetation from smelter smoke. It has been found that plants are much more susceptible to burning from gases during bright sunlight than in diffused light or darkness, and that burning occurs more severely in southern exposures and to the tops of the trees in forests where they are exposed to more intense light than elsewhere. Moreover burning is intimately associated with the assimilative process taking place in the leaves which is at its maximum during bright sunlight.

Susceptibility to burning is also associated with the degree of the developmen[illegible] of the foliage, as is shown by injurious results of illuminating gas in greenhouses and also from smelter smoke. The young immature leaves are generally not affected by illuminating gas while the older ones are burned and killed, and this is probably true of all gases. The nature of the burning from the use of various fumigants in greenhouses varies much as regards species as well as the nature of the substances employed for fumigating. In many instances only the tender edges of the leaves are burned, in others large masses of the tissues are involved, while in others a mere curling of the leaves occurs and burning may be absent.

The burning caused by tobacco fumes, for instance, is different from that caused by volatilized sulphur, which often causes numerous small white spots on the leaves of such plants as cucumbers. This type of burning would seem to indicate that the sulphur gas enters the stomata of the leaves and is there confined, hence causing a local effect. To what extent the conditions of the stomata are involved is not at present known, but numerous observations have shown that most gases close stomata immediately, at least in some plants, although they may subsequently open again, as is indicated by the fact that the transpiration curve suddenly rises shortly after the exposure of plants to gases, after which transpiration or the exhalation of water vapor falls off quite rapidly.

While experiments at the present time do not solve all the factors involved in susceptibility to burning, they do demonstrate that burning from various gases is correlated with the degree of development of the tissue, whether brought about by inferior light conditions or excessive moisture in the soil, the more poorly developed tissue, such as that produced under poor light or relative high soil moisture condition being more susceptible to burning than that developed under conditions more nearly approaching the normal. The moisture content of the air appears also to be an important factor in killing insects such as aphis, as fumigation is apparently more effective in a moist atmosphere than in a dry one.

The most important feature to be borne in mind in fumigation is that it should be done during the night and not in the daytime and especially not during bright sunshine. Furthermore, fumigation should be done only when the plants are in the best condition as regards development. The intelligent gardener can determine at a glance whether his plants are developing normally, and under glass it is possible for him to modify his conditions to a large extent. Special care should be taken not to fumigate during or directly following periods of cloudy weather, as under these conditions the same type of resistant tissue is not developed as on days when the sun is shining. Low night and day temperatures, as well as a decrease in the soil moisture, counteract the bad effects of cloudy weather, but sunlight is the most important factor in developing resistant tissue. There is every reason to believe than any stimulating factor affecting plant development whether physical or chemical in its nature has a bearing upon susceptibility to burning from gases. Fumigation will cause less injury following bright than cloudy weather, and should not be done for two or three days after a cloudy period. By that time the plants will have an apportunity to harden up. The same care and attention should be given to prevent injury from fumigation that is used by lettuce growers in preventing topburn. Top or tip burn, as its name signifies, is a burning of the tender edges of the lettuce leaves and is induced by bright sun on too tenderly developed tissue. Tobpurn in lettuce is, however, counteracted by properly adjusting the heat and light conditions under which the crops are growing, or, in other words, by obviating the production of tender and spindling plants. This is accomplished by utilizing lower night temperature during cloudy periods and also by giving the plants more air either during the night or day time.

A difference of a few degrees in the night temperature has a marked effect in the development of the texture of tissue, especially in a rapidly growing crop. Lettuce growers generally maintain a night temperature of 50° to 55° F. when the crop is first transplanted, at which time there is no danger from topburn. But after the plants have become established and commence to form a head, the night temperatures are reduced to 45° F. or even lower. This night temperature induces a firm texture, and with proper regulation of the day temperature there is no danger from topburn.

Some plants are more susceptible to injury from fumigation than others. Plants with tender foliage or those that have been forced are more likely to suffer injury. The injury to any plant, however, may be greatly decreased or entirely obviated by the due consideration of the conditions of development and the daily periodicity of the plant's activity in the regulation of the dosage and the time of application.

*Journal of the New York Botanical Garden.

The Making of a Six-inch Reflecting Telescope—II.

Instructions for the Amateur Instrument Builder

By C. J. Larson, M.D.

CONTINUED FROM SCIENTIFIC AMERICAN SUPPLEMENT, No. 2179, PAGE 224, OCTOBER 6, 1917

VII. SILVERING

We shall describe the silvering at this point, though in the actual work it is of course best to have the flat ready to silver at the same time.

The silvering method that is undoubtedly best for beginners is the one known as the Martin process. Its simplicity is one of its greatest advantages, and it does give good films. The solutions will be designated A, B, C, and D.

Solution A.—Silver Nitrate, 175 grains, distilled water 10 ounces.

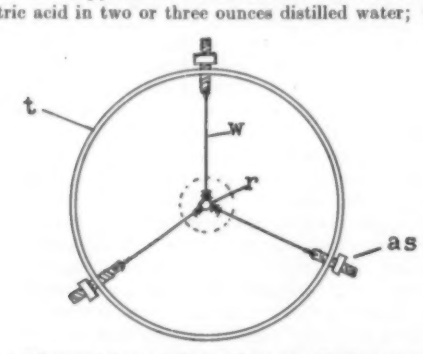
Solution B.—Ammonium Nitrate, 262 grains, distilled water 10 ounces.

Solution C.—Loaf Sugar, ½ ounce. Place this in a flask with five ounces distilled water, dissolve by heating, add 50 grains tartaric acid, boil for 10 minutes in same flask, then allow it to cool; add 1 ounce alcohol. Add water to total 10 ounces.

Solution D.—Caustic Potash, in sticks (of the grade known as purified by alcohol) one ounce; distilled water 10 ounces. Bottle for this solution must have a glass or rubber stopper).

So here are the four solutions. In the actual silvering, equal parts of A and B are mixed, in another graduate equal parts of C and D are mixed; then equal parts of these two mixtures are taken, mixed, and poured over the surface to be silvered. The solution darkens, and after a few minutes a coating of silver forms on the glass as well as on the sides of the vessel or dish used.

Clean the speculum by washing in warm soap water; wash off soap; then wash in solution made of 10 drops nitric acid in two or three ounces distilled water; wash



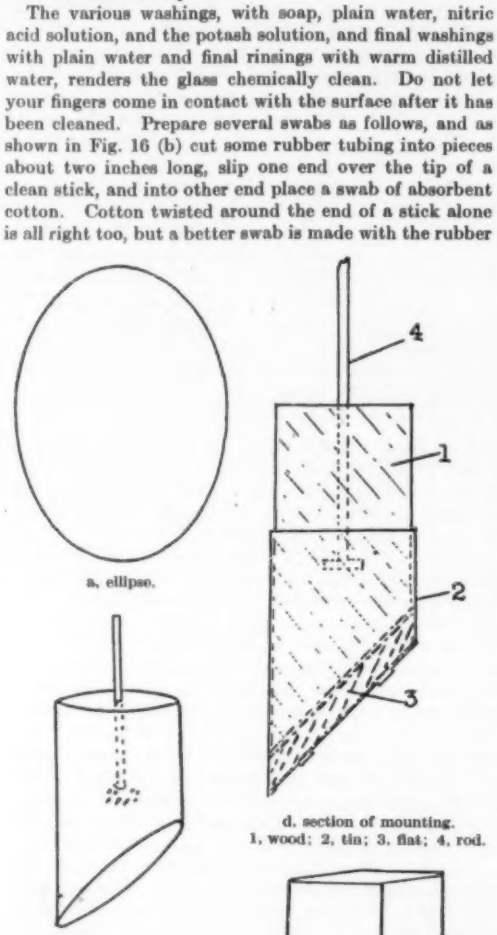
t, telescope tube; r, rod; w, wires; as, adjusting screws.

Fig. 15—Arrangement for suspending and adjusting flat mounting

in water; then in solution made by dropping 10 or 20 drops of the potash solution D into a couple ounces of water, wash in plain warm water, then rinse in two or three changes of warm distilled water, and finally leave it in distilled water of a temperature that feels quite warm when dropped onto the back of the hand, and thus when ready to pour on the silver solution, the disc is both clean and warm. These washing and rinsing operations should be carried out in the utensil in which the silvering is to be done, which dish must be glass, or porcelain, or granite ware. Ordinary water may be used for the washings, but distilled water must be used for making the nitric acid solution, as well as for the potash cleaning solution, and in the final rinsings, and for leaving the cleaned disc in when getting ready to do the silvering.

For a six-inch mirror it is best to use a dish, saucer, or similar utensil; the more nearly the size of the disc the better. You can experiment silvering face down, but I have had better satisfaction face up. If face down, place three bits of sealing wax in bottom of dish and so that these bits support the glass near the edge. With mirrors a foot or more in size it is difficult to get suitable dishes, and for these large glasses a rim of paraffined paper is placed so as to project above the rim of the disc, and thus the surface to be silvered constitutes the bottom of this dish-like affair, and the paraffined paper the sides. But for a six-inch mirror it is better to use a dish. If a utensil of a suitable size is used, one may do as indicated in Fig. 16 (a). Sealing wax in a melted state is run in all around the edge of the disk, preventing the silvering solution from reaching the bottom of the plate as well the bottom of the dish. Some prefer to silver both sides of the mirror, but it is in larger work only that such is needed, if needed at all, because it makes the physical makeup of both sides the same and thus helps to prevent flexures due to temperature changes. If the upper surface only is exposed to the silver, it is evident that only one-third as much silver is used as if upper and lower surface of disc as well as the bottom of the dish are covered with the deposit.

The various washings, with soap, plain water, nitric acid solution, and the potash solution, and final washings with plain water and final rinsings with warm distilled water, renders the glass chemically clean. Do not let your fingers come in contact with the surface after it has been cleaned. Prepare several swabs as follows, and as shown in Fig. 16 (b) cut some rubber tubing into pieces about two inches long, slip one end over the tip of a clean stick, and into other end place a swab of absorbent cotton. Cotton twisted around the end of a stick alone is all right too, but a better swab is made with the rubber



a, ellipse.

d, section of mounting.
1, wood; 2, tin; 3, flat; 4, rod.

b, wood cylinder.

e, wood block used with rectangular flat.

c, shape of tin and flat.

f, corners cut for 8 sided flat.

Fig. 14—Mounting of flat

tubing. Have several ready and lying on a clean paper. Also have a good sized pitcher of warm water ready, and a smaller one with slightly warm distilled water. When trying the temperature of the water, do not put your fingers into it; pour a few drops onto the back of your hand. If the cleaned disk is left in the warm water it is ready for the silver; just to pour off this water just before pouring on the solution. The solutions may be room temperature, and the room should be about 72 degrees.

If disc is arranged so upper surface only is exposed to solution, take ½ ounce of A, ½ ounce of B, mix them. Take ½ ounce of C, and ½ ounce of D, mix them. This latter will turn slightly straw colored, especially if standing a few minutes. Mix the two mixtures, and in a few seconds this last formed mixture will begin to turn brown, then darken, and as it begins to darken pour it over the plate. Do not pour off the warm distilled water from the plate until you are ready to pour on the silver solution, because the plate must not be allowed to get dry even in a little spot, nor should it be allowed to get cold. The mixture made by combining solutions A and B, (call it mixture No. 1), and that made by mixing C and D (mixture No. 2), may stand a while, but of course Nos. 1 and 2 are to be mixed only when ready to silver. The glass with the water in which it is immersed cools rapidly, and it is advisable to have mixtures No. 1 and No. 2 ready, then test the temperature of the glass and water, and unless the water feels quite warm to the back of the hand, add carefully and gradually enough hot distilled water to bring up glass and water to the proper temperature. Then pour off water, mix 1 and 2, and as the solution darkens, pour it over disc, rock continuously, and soon the solution turns black, and a silver film forms over the mirror. Watch for the change where the liquid seems to begin to curdle; little specks of precipitate the size of pin points appear, the liquid part resembling to some extent "whey." At this point pour off the solution, wash with several changes of distilled water (of room temperature). After a couple washings with the distilled water, one must look after the specks of precipitate that may be left from the solution. Cover the mirror with water, and then go over the surface very carefully with a lightly held cotton swab. Have the cotton loose and hold it so the cotton trails over the surface, but be careful the stick or rubber tube used for holding does not touch the film. Then run on *cold* distilled water, as cold as you can get it, and leave it in the cold water several minutes; then after leaving in cold water as above directed, pour off water, and wash over with several small changes of pure alcohol, which rids it of all water, then place it so it can drain and dry. As stated at the beginning of the paragraph, if the one side only of disc is exposed, the quantities of solution indicated are sufficient, but if bottom of disc as well as bottom of dish are exposed to the solution, there is ap-

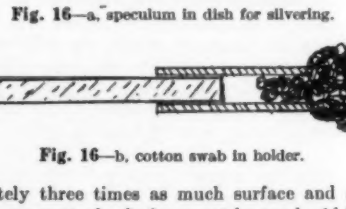


Fig. 16—a, speculum in dish for silvering.

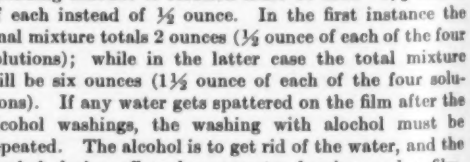


Fig. 16—b, cotton swab in holder.

proximately three times as much surface and a corresponding amount of solution must be used—1½ ounces of each instead of ½ ounce. In the first instance the final mixture totals 2 ounces (½ ounce of each of the four solutions); while in the latter case the total mixture will be six ounces (1½ ounce of each of the four solutions). If any water gets spattered on the film after the alcohol washings, the washing with alochol must be repeated. The alcohol is to get rid of the water, and the alcohol drains off and evaporates leaving a dry film. Water drops will leave spots after drying off.

After the film is dry, place it in a warm place, preferably where the sunlight strikes it; and leave it there for 24 hours or so. It may have a peculiarly foggy appearance, but this all comes off in burnishing. Take a soft chamois skin, roll up a wad of absorbent cotton about the size of a small walnut, and makes a pad for burnishing. The pad should also be warm, same as the mirror. Dip the tip of the pad into the rouge (to be described later) and start to gently polish the film in small circles, about an inch or so. Notice carefully if there seems to be any scratches from the pad, and if so rub off the rouge with a bunch of cotton, dip into some more rouge, and try again. Take small areas at a time thus, and it won't take long to get over the whole surface, and if all is right, a beautiful reflecting surface results. Do not touch any part of the film with the fingers, nor any part of the glass surface before silvering. And when you are exhibiting your telescope to your friends, particularly the mirror and flat, ask them at the outset not to touch the silvered surfaces. Finger marks are very hard to remove, often impossible. Some people simply cannot refrain from rubbing their fingers or hands over any and all bright polished surfaces.

The rouge used for burnishing should be the very finest jeweler's rouge, and should be water separated,

same as that used for the last polishing of the speculum before silvering. Rouge is red oxide of iron, and will not cut glass, any cuts or scratch marks that may occur being due to grit particles of some sort getting into the rouge. Rouge is not expensive, the finest quality even being quite cheap. Place some of it in a "conical" sedimentation glass if such is available, if not a test tube or any tall, narrow container, add water and shake around or stir with a clean stick or glass rod, and then set aside for an hour or so; carefully siphon or pour off the clear water, and if the rouge is to be used for the polishing of the speculum before silvering, it may be taken in the pasty form directly with a clean spoon and smeared over tool or disk, but if for the burnishing of the silver film, it must of course be dried. The grit particles settle to the bottom of the jar or tube, and care must be used in removing the upper layers so as not to get any of the grit that may be in the bottom layer. The dry rouge desired for the burnishing is best obtained by removing a few teaspoonfuls of the upper layers of paste and placing in clean dish or saucer, and a few hours drying in an oven or on a radiator or stove will produce the desired result. It must be kept carefully covered so no dust or anything else that may contain grit can get into it.

Some authorities state that after this or that method of silvering has been carried out, the film need not be burnished, being ready to use as it is. I have not seen a film that was not improved by burnishing, and sometimes an extremely foggy film, one that appears to be a failure, will turn out excellently after the treatment with the rouged pad. Exposing the film to sunlight and keeping warm for a day or so seems to make the silver ever so much more adherent, and often so hard you simply cannot rub it off. Cuts from grit in the rouge are a different proposition however. If badly cut up from such a cause, it is better to resilver. Films are easily removed with nitric acid, which may be poured in small quantity over the glass and by carefully using a swab of cotton all trace of the old film is easily and quickly removed. Care should be used in handling nitric acid as it makes bad burns of skin and clothing, and every trace of it should be removed from all parts of the disc and utensil before silvering. Keep some ammonia on hand, and apply it immediately to any burn or any spot on clothing. Better use old clothes in this work, or else a good big apron, or a suit of "overalls."

In silvering formulae one often reads of the *two* solutions, one of which is referred to as the *silver* solution, or sometimes as the *nitrate* solution, and the other the *reducing* solution. This is because in practically all methods of silvering, regardless of how many solutions are originally made up, there are finally two mixtures that are to be united in the final step, and one of these contains the silver, while the other contains the sugar that with the aid of other elements acts as a *reducing agent*. The silver is in solution in the form of a salt, usually the nitrate. In the reduction, changes occur of a nature to take away the elements with which the silver is combined, and free, or metallic, silver results, and this deposits on the glass surface.

In the formula here described four solutions are made up, these being designated A, B, C and D, but for the final step there are but two solutions or mixtures to be combied before pouring onto the plate. A and B are mixed and that mixture we styled mixture 1, and is the "silver," or as some call it, the "nitrate" solution. C and D when mixed constitute the "reducing" solution. C contains the sugar and D is the caustic potash. These turn straw color when mixed and if allowed to stand some time will turn yellowish brown. But these solutions should not be mixed until one is ready to use them.

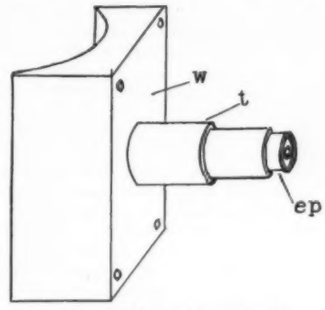
Do not confuse this darkening of the mixture made by combining solutions C and D with the darkening of the final solution that is poured on the glass. When A and B are mixed there remains a colorless liquid. When C and D are combined a straw color develops in a few minutes or even a few seconds. That is all right. But if this solution of C and D stands for a half hour or more it may get very dark brown, and it should not be allowed to stand long enough for that. Now when you are ready to silver and you combine the mixtures (1 and 2), wait until you see that this final solution is beginning to turn dark brown (which it will usually do in a few seconds) then pour it on the glass. It will get darker and then silver will show up. Stop the chemical action when the little curds of precipitate appear and the rest of the solution assumes the appearance of "whey." Pour off the solution quickly and pour on distilled water, or the action may be stopped by pouring on a large amount of water, this having the effect of so diluting the solution that action stops, but this water with the remains of the solution must then in a few seconds be poured off and clean water run on.

When you have mastered the problem of silvering, you will enjoy it as much as the making of any other part of your telescope. Should you never have occasion to do another job of silvering, you will never regret having learned this most fascinating experiment in chemistry.

VIII. THE DIAGONAL FLAT AND ITS MOUNTING

Let us discuss the diagonal flat. That shape of flat is best and that mounting is best, that least obstructs the light rays to the speculum below it, while permitting the greatest reflection of light rays from the mirror to the eyepiece. A right-angled, "internal reflecting" prism was once considered the best, but the amateur cannot make a prism and they are expensive to buy. Optical glass must be used in a prism, as in all cases where light has to pass through glass. The silvered plane is the most practical.

For the flat select the best piece of plate glass you can find. The thicker glass has as a rule a more nearly true plane surface, but otherwise there is no need of very thick glass for this part. This piece must be free of cuts



w, wooden socket; t, tube; ep, eyepiece.

Fig. 17—Mounting for eyepiece tube

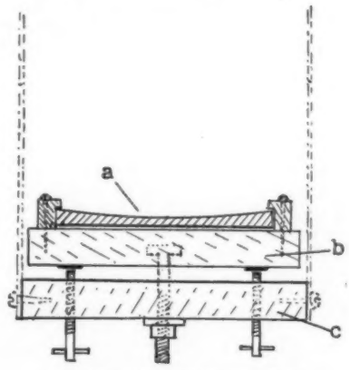


Fig. 18—Mounting for speculum

a, speculum; b, wooden cell with cleats; c, bottom of telescope tube, with screws for adjusting speculum.

or scratch marks. If you have a glazier cut the discs for mirror and glass tool, it might be advisable to have him cut the flat also. Caution him to select a scratch free surface for the flat. It makes no difference if there are marks on the discs as they will be ground out. But remember that the flat is to be silvered with the surface plane, and it would be impractical for the amateur to try to grind or polish out the surface of the flat. If you get these parts of a glazier remember to get some small pieces of any shape for experimenting in silvering.

The flat should be elliptic in shape and bevelled at the ends so as to have no glass extending outside of the actual reflecting surface. If a rectangular flat is to be used it should be a little over one and one-half times as long as wide. By cutting off the corners as shown in Fig. 14 (f) an approximation to the ellipse is secured. Take a wood cylinder of about 1¼ inch diameter. Cut off one end at an angle of 45 degrees (best done in a miter-box). Cut the other end square, and have the part about three inches long on the longest side. A piece of curtain pole is very useful for this, such being about 1¼ inches in diameter as a rule. If a piece of brass or copper tubing of this size is obtainable, one end is cut off at an angle of 45 degrees, the same as the wood. This section of tubing should slip over the wood cylinder. If too large it may be made to fit by wrapping a layer or more of strong paper around the wood, or by splitting the brass tube and cutting out a section of such size that the tube fits when the edges of tubing are taken up. If tubing is not obtainable a piece of tin answers very well, First wrap a piece of writing paper around the cylinder and mark or draw a line on the inner side of the paper around the edge of the elliptical surface of the wood where it is cut at the 45 degree angle. This paper is removed and cut at the line marked. This is used as a pattern for cutting the tin, except that four or five little projections are left, about ⅛ inch wide and about the same length. These are to be bent in over the edge of the glass flat to hold it in the mounting. The tin must extend down further than the edge of the wood, by an amount equal to the thickness of the glass flat. Fasten with small screws or tacks. The flat is put in place and the little projections are turned in over the edge of the flat, and thus hold it securely.

Great care should be exercised at all times when handling the glass parts, whether unfinished discs or silvered speculum. Sometimes glass cracks very easily, while at other times it is surprisingly strong. Dropping the part or letting some object fall onto it should be guarded against. When wet the glass discs are very slippery, and aggravating accidents may occur. In working the edges of the disc or flat, whether with cutter, pliers or on the grindstone, a slip may cause serious injury. It may be advisable to provide some protection to the surface. Strong paper may be glued on or a piece of cloth material may be similarly used. It is easily removed by soaking in warm water. And even then one has to be careful to see that it does not come in contact with any hard or sharp object. Practise cutting elliptic or rectangular planes out of any rough scrap glass, and working and grinding the edges. Then when you feel you have learned to thus work reasonably well you may tackle the real work.

Get a bolt, ¼ x 6 or ¼ x 8, drill a ¼ inch hole through the center of the wood, the lower end of this hole being bored out to permit counter-sinking the head of the bolt. Run in melted sealing wax or put in a wood plug to fill the space after the bolt head is in place. Cut six wires 10 inches long. Get six light bolts, and attach one end of each wire to the head of each bolt. Three of these wires are attached to the rod near its entrance into the wood, while the other set of three wires are attached to the upper end of the rod. A reinforcing band should be placed around the telescope tube where the lower set of bolts comes through, and another where the upper set emerges. The wires are adjusted at their attachments so that the rod is as nearly as possible in the center of the tube, and in line with it. Then by turning the nuts on these bolts the proper tension of the wires is obtained, and by lessening the tension on one while increasing that of the opposite ones the needed adjustments are very accurately obtained.

Of course the cylinder of wood and the piece of tubing are used only if the flat is elliptical. If the flat is made rectangular, a square piece of wood is required, and if the corners are cut from the flat, making it eight-sided, the wood should be eight-sided. If the beginner finds it very difficult to make an elliptical flat he may use the four-sided or the eight-sided and later as he finds time and is more experienced in working with glass he may make an elliptical flat and the corresponding mounting.

The optical parts may be mounted in a tube or in a skeleton framework. For a six-inch telescope a tube is better, and the average tinsmith can make a very suitable one of galvanized sheet metal, though two or three sections of seven-inch stove pipe will do in the absence of anything better. If not sufficiently rigid, reinforcing strips of strap iron or even strips of wood may be added. and also bands at the ends. Bands are usually needed too where the bolts that serve to secure and adjust the diagonal mounting come through the tube. Near the lower end of the tube there should be an opening or door to permit access to the speculum—placing it in or removing it, as well as placing and removing the cap that should be over it when the telescope is not in use.

For the eyepiece tube, the amateur if he wishes can make an arrangement like that shown in Fig. 17, which may be a little crude in appearance, but easily made and will be found to work quite well. A rack and pinion focusing adjustment is very convenient, but the material may be hard to get unless one has access to the stock of some big city hardware store. Most astronomical eyepieces are 1¼ inch outside measure, while the microscope eyepieces previously mentioned are 23 millimeters (23/25 inch) outside. So it is advisable to make the drawtube to take 1¼ inch eyepiece. An adapter that fits into the 1¼-inch drawtube and takes the 23/25-inch eyepiece, is made, and thus one can use the low-priced microscope eyepieces, or the regular 1¼ inch celestial eyepieces interchangeably in the same drawtube. This adapter may be nothing but paper wound evenly on the smaller diameter ocular until it fits snugly into the 1¼-inch drawtube. Focussing is accomplished by moving the drawtube in or out in its socket, which is attached to the side of the telescope tube on a level with the diagonal flat. This socket can be cloth lined and by having a proper fit, the tube can be moved in or out easily and still be a sufficiently snug fit to remain in focus.

There are many ways of mounting and adjusting the speculum, but I shall describe just one of many ways that requires nothing but the commonest materials and

but little labor. Cut a wooden disc—pine will do if free from warp or shake—about one inch thick and ¼ inch less in diameter than the inside of the telescope tube. Drill a hole in the center to take a ¼ inch or ⅜ bolt, and countersink the head. Make three cleats as shown in Fig. 18, of some strong tough material as mahogany or oak, taking the precaution to drill the screwhole in the block of material before fashioning it into the cleat, thereby lessening the possibility of splitting it in drilling. A piece of felt should be placed under the glass and a little piece of the same material under the stop of each cleat, and the screws should be drawn down carefully so as not to endanger the glass. The construction that holds the speculum is usually referred to as its "cell." A cap that fits over the cell should be provided and the maker can exercise his own ingenuity in making it. This cap, or some other protection, should always be provided and in place when working with the flat, or its mounting, or when doing any work where something may accidently drop down the tube and injure the mirror.

Now you have the mirror mounted in its wooden cell, and have the telescope tube ready, but as yet no arrangement for holding the mirror cell; and it must be mounted with provision for adjusting and focusing it properly. This problem may be solved in a number of ways, but the method here described entails but little labor and calls for nothing more than a wood disc and few stove bolts. Cut a circle an inch thick and of a diameter equal to the inside of the telescope tube. Bore a hole in the center to take the end of the bolt projecting from the lower side of the mirror cell, and three other holes equidistant from each other and about 2½ inches from the center. If of mahogany or oak or some other hard wood, ¼ inch holes may be bored and then ⅜ inch bolts may be threaded in and out, cutting a thread in the wood; and then run some melted paraffine through the holes and then thread the bolts in and out a few times again, and a very useful job results. String a few washers on the bolt projecting from the mirror cell, and push it through the central hole of the wooden disc, and draw up loosely with a nut. Insert the three adjusting bolts, and by screwing these in or out as needed the entire mirror mounting can be very nicely adjusted to focus the light on the diagonal flat. Little metal plates may be fastened to the bottom of the cell where the ends of the adjusting bolts turn against it, or a circle of thin sheet iron covering the entire lower surface may be attached. This wood disc of course is placed in the bottom of the tube and a few screws through the sheet metal and into the edge of the wood will hold it.

[TO BE CONTINUED]

The Parachute Up-to-Date

CHRONOLOGICALLY speaking the parachute is the pioneer type of aircraft, being generally acknowledged to have antedated the invention of the balloon. It is on record that several hundred years ago, Siamese royalty were entertained by an inventor who made leaps from great heights with parachutes attached to his body. That versatile Italian, Leonardo da Vinci, who appears to have anticipated a great many modern "inventions," described a parachute as long ago as 1514, but so far as can be ascertained, the first authentic practical demonstration of the utility of the parachute was made by a Frenchman, M. Lenormand, at Lyons, in 1783. This inventor suggested the parachute as a means of descending from the upper parts of a burning house, and actually succeeded in making a safe descent from the top of a building in Lyons. In 1783, the aeronaut Blanchard conceived the idea of using the parachute in ballooning and first tested one by attaching it to a basket in which was placed a dog. The descent was made safely, but when Blanchard tried the experiment himself, although the fall was certainly very much retarded, he sustained a broken leg. This happened in 1793. On October 22d, 1797, M. Garnerin made a very successful parachute jump from a balloon at an altitude of over a mile. With this famous descent it may be said that the parachute was fairly established as a means of checking the speed of a free fall from a height, and has been employed in this manner for a variety of purposes since that date.

Fundamentally, there is no very great danger attending the descent in a parachute, provided the first essential condition is satisfied, *i. e.*, that the parachute opens. Unfortunately, there is, or rather, there was, always the possibility that the many cords which support the parachutist from the periphery of the parachute may become entangled and prevent if from opening. More than one life has been lost in this manner, and this uncertainty has, without doubt, been largely responsible for the suspicion with which many, one might perhaps be permitted to say, most modern aviators regard the parachute. In times of peace, when the possibility of breakage in the air is remote, it has been rightly argued that in most cases the aeroplane itself would form the best parachute the pilot could possibly have, as it is superior to the parachute in that it may be steered in any direction, thus enabling the pilot to choose a suitable landing ground, whereas in the parachute he must perforce go whither the wind chooses to send him. In times of war, however, it is unfortunately no infrequent occurrence that an aeroplane gets so badly damaged by gun fire that a portion of it, such as a wing or a tail boom, breaks away. Marvellous escapes in such cases are on record, but, generally speaking, it means certain death for the unfortunate pilot. It is quite obvious that if a parachute can be made which is absolutely certain to open when required to do so, and can be made to leave the aeroplane without catching anywhere, and tearing itself to pieces, an opportunity to save himself has been afforded to the pilot of a broken machine. Of what has been done in the way of parachute descents from aircraft during the war it is not, of course, possible to speak at present, but previous to the outbreak of war it may be remembered that Pegoud jumped out from an aeroplane with a parachute attached to his body and landed safely, the Blériot monoplane on which he made the experiment landing itself in a practically undamaged condition near by. In this country the parachute jump from an aeroplane was first made from a Grahame-White biplane by Mr. Newall, who was seated on one of the chassis skids. It may still be remembered how the parachutist continued to postpone the jump until the late Major Goodden, who was himself an experienced parachutist, became impatient, and, climbing out on one of the wings of the aeroplane, kicked his passenger into space. The landing was effected without a hitch, and thus ended the first parachute jump from an aeroplane in England.

With the extensive employment of airships, kite balloons, and spherical balloons, the parachute has become a necessary accessory, and its usefulness for these purposes is probably established beyond question. Slower, considerably slower, has been its adoption for use on aeroplanes. This is probably partly due to prejudice, created by the erratic and uncertain behavior of the old-fashioned parachute, and partly to the inherent difficulty of successfully launching it from such a fast moving vehicle as an aeroplane. In the first place, the parachute must be so placed in the aeroplane that if anything goes wrong with the machine, the pilot has but to release his safety belt and jump overboard, and the parachute will release itself from the machine and open in a minimum of time so as to reduce the length of "free fall" experienced by the pilot. Secondly, the possibility of the parachute opening before it is clear of the machine, and, therefore, likely to be blown back and catching in the tail planes or tail skid, must be absolutely eliminated. Finally, on reaching the ground the pilot must be able to detach himself instantly from the parachute, so as to avoid being dragged along the ground and through ditches, hedges, etc.

From a visit to the premises of Messrs. E. R. Calthrop's Aerial Patents, Ltd., at Eldon Street, in the City, we have received the impression that all these desiderata have been attained in the latest type parachute designed by this firm. It is now a good many years since Mr. Calthrop commenced his experiments on parachutes, and if he has at last succeeded in turning what was once a very erratic craft of the air into a machine designed to perform a given function with certainty, and no others, it is only after endless experimenting and improving upon improvements, that he has now succeeded, we firmly believe, and so far as we are able to judge, there is none of the old-time shut-your-eyes-hold-your-breath-jump-and-see-what-happens uncertainty about the Calthrop parachute.

Under present conditions, it would obviously be grossly indiscreet were we to give a detailed description of the Calthrop parachute, but a brief outline of what the parachute is claimed to do, and does, as evidenced by the series of moving pictures shown us, may be permissible. The chief advantage of the Calthrop parachute lies in the ingenious method by which it is released from the craft supporting it. This is at once simple and effective, with no possibility of getting out of order. A pull of given magnitude, furnished by the weight of the parachutist, releases it from its anchorage, and as soon as the supporting tapes are taut the parachute opens and sustains the pilot. The method of folding the rigging has been very carefully thought out, and the result is that, instead of tumbling out in a bunch with the attendant possibility of becoming entangled, the rigging is unfolded gradually, so that each of the strands is taut and straight for the portion of it that is at that moment outside the covering. The pleating and folding of the parachute body itself is carried out in a way that has been found by experience to give the least amount of friction between adjacent folds during the process of opening.

Incorporated in the rigging is a shock absorbing device which has been found to be so effective that the pilot did not experience any sudden jerk whatever when the full weight of his body was taken by the parachute. From a series of very interesting films it appeared that the longest time taken for the parachute to open after the pilot had jumped from the aeroplane was 2½ seconds, the time taken depending, apparently, upon the speed of the aeroplane, the quickest opening occurring with the fast machine and the slowest with a slower machine. As regards the altitude from which it is safe to jump, Mr. Calthrop advocates 200 feet as the minimum, although we missed a film, showing a jump from only just over 100 feet., in which the pilot was being supported by the parachute while still a matter of some 20 or 30 feet from the ground. The quick release by means of which the pilot detaches himself from the parachute on landing is of the very simplest type, and its effectiveness was clearly demonstrated in one of the films, which showed that the pilot got into an air current when only a few feet from the ground, the parachute trailing out at what appeared to be a very dangerous angle. However, by releasing himself just before striking, he landed safely, and the parachute came to rest as soon as released of the weight of the pilot. That this is a necessary precaution was illustrated in another film, in which the weight was in the form of a sand bag. A fresh breeze was blowing, and the bag trailed along a considerable distance before the attendants were able to capture the parachute.

Air and water-tight covers protect the parachute when not in use, and absolutely prevent it from getting damp. To ensure that nothing is tampered with in transit, the parachute is packed by the makers in a strong wooden box, which is then sealed, a card of instructions for use being tacked inside the lid of the box, to be carefully read before touching anything of the contents, which is all arranged according to the makers' system, even to the pilot's harness. Certainly, if careful design and painstaking workmanship count for anything, the Calthrop parachute is as near perfection as it is possible to get it, and the fact that a perusal of the, unfortunately, only too frequent accidents to aviators would indicate that a very great percentage of these pilots might have been saved had they been equipped with a reliable parachute, is a very strong point in favor of a more general adoption of this useful "accessory." We do not doubt that there are a good many people who shrug their shoulders at the idea of a parachute on board an aeroplane. We were inclined to do the same—until we saw Mr. Calthrop's "Guardian Angel."—*Flight.*

Factors Influencing the Condensation of Aqueous Vapor in the Atmosphere

EXPERIMENTS on the condensation of water-vapor in the air under different conditions give the following results: The formation of the nuclei which, besides dust, may provoke the condensation of atmospheric aqueous vapor is determined especially by the presence of ozone, nitrogen peroxide, and, indirectly, ammonia. Electrical discharges, flames (independently of their fumes), and glowing bodies favor condensation in so far as the above substances are formed in their neighborhood. The property exhibited by some substances of disturbing fumes in the air is identified with the phenomenon of deliquescence, the latter property being manifested without the surrounding medium being saturated with moisture. The conception of a medium saturated with vapor is, at any rate in practice, of relative and not absolute character. This relativity is implied by Kelvin's law, according to which the condensation or dew-point depends not only on the vapor pressure, but also on the radius of curvature of the surface of bodies in the immediate neighborhood of particles of vapor; it must now be extended to the consideration of the nature of these bodies and of their distance from molecules of the vapor.

Contrary to the conclusions of Lenard and Ramsauer [Abs. 441 (1912)], the action of the ultra-violet light is not necessary for the formation of the nuclei and functions only as a source of ozone. Further, gaseous ions exhibit no power to constitute condensation nuclei, so that ionization of the air by either radio-active substances or Röntgen rays has not been shown to be a necessary or sufficient cause for condensation of supersaturated aqueous vapor.

Trees, especially tall ones and those rich in resins, give rise to ozone, and should therefore favor production of rain. Opinions on the actual influence exerted by trees, are, however, very variable.—Note in *Science Abstracts* on an article by A. MASINI in *N. Cimento.*

Correspondence

[The editors are not responsible for statements made in the correspondence column. Anonymous communications cannot be considered, but the names of correspondents will be withheld when so desired.]

The Speculative Value of the Corpuscular Theory

Editor SCIENTIFIC AMERICAN SUPPLEMENT:

Late researches and demonstrations in the domain of exact science, together with observations, gleaned from applied science, have played havoc, to a certain extent, with our supposed knowledge of and insight into nature. Many a thesis, regarded hitherto as axiomatic, has been demolished, innumerable notions have been uprooted; others have to be corrected, modified or amended; one hypothesis after the other had to give place to newer ones.

There were particularly two main factors in bringing about this upheaval: (1) Our more intimate working acquaintance with electricity (though our knowledge of what it is remains still next to nothing), and (2) chiefly the discovery of radioactivity.

Ever since the downfall of transcendental idealism, men who feel the need of supplying patient mankind with a brand new "View of the World" are prone to take hold of some recent hypothesis in natural science and use it as a foundation whereon to build their metaphysical superstructure. It certainly would have been in the nature of a miracle, had none of those speculative minds availed themselves of the brilliant opportunity offered by Rutherford's Corpuscular theory.

The features of this theory are set forth in the article by J. C. Whiteborn in the SCIENTIFIC AMERICAN SUPPLEMENT, Nos. 2171 and 2172, of August 1 and 18. The Nature of Matter theory, as well as from researches, calculations, quotations by and from Rutherford, Perrin, Thomson, Kaufmann, etc., is: The Universe is Kinetic Energy, or, as he somewhat redundantly puts it: Active Energy in Motion. With this conception of the universe, he thinks, he can make the wolf dwell with the lamb, make the lion eat straw like the ox, and prevail on Ephraim and Juda to stop fighting one another, in other words, bring about "a definite, thorough and complete reconciliation of Science, Theology and Philosophy."

We may remark in passing that the conception of an active energy (though of course not a radio-active one), as the free and first cause of everything, is as old as the hills, has never ceased to be dished up, either plain or in various sauces, yet never did it assuage the bitterness of that three-cornered fight.

In order to ascertain how warranted Mr. Whitehorn is in his expectations, it behooves us to examine first of all, if and how far our understanding of the universe has been illuminated and clarified by the new theory of matter; what new light does the electron theory shed upon Cosmology and Ontology? What point of vantage does it present for metaphysical speculation that has not been occupied before?

THEORY OF MATTER, OLD AND NEW.

To answer these questions intelligently let us state and compare the two theories. The old hypothesis briefly is this: Matter is composed of small particles. Those that cannot be divided any further by mechanical means, are termed molecules, those that cannot be divided any further, neither by mechanical nor chemical means, are called atoms; they aggregate, because of the law of affinity, they are held together by attraction; crowding is prevented by repulsion. We may note here that Kant as early as in 1755 had explained *a priori*, the evolution of cosmic matter and the formation of solar bodies by the law of attraction and repulsion.

The new theory, as stated by Mr. Whitehorn reads: "Matter is composed of electrical charges, which, by their motion give to the bodies their mass."

This formula is probably good enough for the requirements of the physicist who limits himself to his specialty, but not for the philosopher in quest of light on transcendental problems. The statement lacks clearness, precision and logic. Mass is a necessary predicate of body, there is no body without mass, hence mass cannot be imparted to the body, because this would imply a previous stage, where the body had no mass, which is tantamount to saying, when the body was not a body. Mass is just as inseparable from our conception of a body as liquidity is from our conception of water. True there are non-liquid combinations of H_2O, but for them we have separate conceptions: snow, ice, vapor, steam.

Hence we have to discard the author's formula, but we can glean from his article the following statement:

Matter is composed of electrons, which together with some unknown positive charge form atoms, which in turn by their mutual attraction form molecules, which by dint of intermolecular forces aggregate and form bodies.

The whole difference, then, between the old and new theory of matter is a different name for the smallest, *i. e.*, further-indivisible particle. A difference wholly negligible for our errand. About of the same import to us, as the information that, while the ha'penny is the smallest coin in England, they have the still smaller farthing in Scotland, would be to the economist, trying to grasp the magnitude of British capitalism and finance.

We have had atomistic systems galore since the days of Anaxagoras (+430 B. C.), with atoms and monades and Herbart's reals and what not, yet we did not make any headway, and it is doubtful if the electron will be of any better help to us.

We have to consider now some other salient feature of that part of Mr. Whitehorn's article wherein he quotes authorities:

(1) Rutherford traced Radium back to Uranium and onward to lead. Very interesting but without bearing in our investigation. If Radium could be traced back again from lead to Uranium, it might furnish a basis for eschatological meditations.

(2) Transformation of matter.—An atom of one element can be changed into an atom of another element by the loss or gain of one or more electrons. This does not conflict with the old view. Mr. Whitehorn, notwithstanding. It taught: All matter is alike and varies only in density (specific gravity). By altering the density you may change the form. The changeability of a substance hitherto regarded as an element proves nothing but a wrong classification.

(3) The Beta, as well as the Cathode Rays, are able by increasing their speed to increase also the mass of their carriers (Beta or Cathode particles). This, at last, opens a vision, "a truer understanding of the universe as being all in one, logical, purposeful and sublime."

HIC RHODUS, HIC SALTA.

Now will Mr. Whitehorn have the courage of conviction and give us a clear, succinct, unequivocal cosmology, somewhat like this: In the beginning was the Emanation; the primordial Ray, by increasing its speed, increased the mass of its primordial carrier, until that mass became big enough to build the world from.

Yet this cosmology could hardly claim originality, for emanation as first cause was taught already by the Neo-Platonist, Plotinus and Troclus, also by the schoolman Scotus Erigena, all three, of course, hypostatising the radiating first principle. Our author had better follow suit, else he will never escape the embarrassing question: Where did that emanation come from? We are still confronted by another difficulty, Emanation, according to scientists, is not a synthetic process but rather one of disintegration, of disassociation, which conditions an anterior integer, an anterior association. But how can the posterior be first cause? Or does Mr. W. teach a primordial Uranium? Or shall we have to change the opening paragraph into: In the beginning was the Pitchblende?

A LIGHT THAT FAILED.

But much as it is to be regretted, Mr. Whitehorn does not venture upon a like-bold declaration, like those, but limits himself to some vague hints about a true philosophy, that clings close to empirical knowledge and yet gives emotions a rightful place, without affording us the least outline. He, however, is positive of two things:

(1) "This is not a universe of dead atoms propelled by blind force, but a universe of active energy." (2) "Now we see, with clearer light than before, the weakness and folly of materialistic philosophy. *We know now that matter is not an ultimate entity.*"

Let us note for further reference, that blind, in the connection as above, can but mean: unintelligent, unreasoning, aimless; we must confess our ignorance as to whom his challenge is directed against. That substance (matter) is inert and dead, has been a thesis of Cartesian and Rationalist philosophers, but they never assumed a blind propelling force but taught expressly, God gives impetus, motion and life to the substance. Besides these systems have been exploded and are dead long since. Nor could that challenge be aimed at materialism. I have read all its prominent exponents of this century and the three preceeding ones, from Toland, Hartley, Cabanis, Moleschott, Vogt, down to Haeckel and Plate, but nowhere have I encountered the theory that this is a universe of dead atoms. Toland (1710) arguing against Descartes' extended substance, asserted:

"Matter is an active substance, that is, *Force.*" Buechner in his Kraft and Stoff (1855) teaches expressly: "No matter without force, no force without matter." The essence of the materialistic doctrine, by the way, is the rejection of all preternatural and supernatural ideas. "Natural laws comprise all that we are able to fathom," (Plate.) To some superminds a confession like the foregoing may sound "weak and foolish."

ULTIMACY AND MIRAGE.

Materialism, moreover, asserts the permanency and reality of matter, which according to Mr. W. is an additional proof of its foolishness and weakness. "Matter is not an ultimate entity."

Prior to the discovery of radio-active substances, the assumption "nothing gets lost in nature," was shared by natural scientists and philosophers alike. The hypothesis: while matter may change its forms, while its component parts may separate and scatter widely, yet the sum and total of their quantity will remain intact, is based upon some observations (for instance, upon the weighing of steam), but can neither be proved nor refuted by demonstration. Obviously, we cannot hunt up and reweigh all the scattered parts and if we could, in order to go safe, we would have to reweigh incessantly all balances and weights.

Now, however, we are told that matter can and does get lost, as proved by the behavior of radium and other radio-active substances. This demonstration, as a matter of course, can only apply to radium, etc., in the laboratory, for elsewhere we can only prove the shrinkage but not the loss, *i. e.*, annihilization of any matter. Now the laboratorist states his case: "After making the receptacles as absolute a vacuum as we know how, we sealed it hermetically as soon the radium was put in; but now half of the radium is gone, has vanished, is lost. Ergo, matter is perishable."

It is upon similar testimony, and because some laboratorists are at a loss how to explain a shrinkage in the contents of a bottle, we are bidden to discard a rational and hitherto trustworthy hypothesis. Meteorites have fallen and keep on falling in defiance of Newton, yet no one asks us to declare the law of gravitation invalid. But here, on less tangible evidence, we are told to jump at an unreasonable conclusion.

If the laboratorist were to consult his cook in the matter, the chances are she would suggest, that the container leaks. An explanation which the laboratorist, probably rightly, will not except, but it may dawn upon him that the inseparable twins—ray and particle—can penetrate the walls of the container, be they glass, metal, or china, in other words, make their escape.

Now it is incumbent upon the laboratorists to demonstrate that this escape by penetration is impossible. The task will be a herculean one. For among other things they would have to prove, that all radiating argons, as found in minerals like lead or pitchblende, belong to the original components of that mineral, that they did not get into it, after it had been formed by its other component parts, that the argon, so to speak, was not an organizer of the lead or pitchblende corporation, but only a late shareholder. For failure to prove this would take the ground from under their feet. If the argon can get into the mineral way under ground, it certainly should be able to get out of the glass bottle in the laboratory. But what would the radio specialists among the physicians say were their brother chemist or physicist to declare, Radium can not penetrate its container?

Although it is not incumbent upon us to suggest any explanation of the missing matter, we may mention the possibility, of some insects feeding on beta particles. The luminosity of the glow-worm then, would no longer be a puzzle. Some birds may owe the brilliancy of their plumage to feeding on those insects or on emanation direct. I leave it to others to elaborate this glorious hypothesis waiving even, most generously, the priority to copyrighting.

Scientists tell us Uranium, Ionium, etc., have a "life" of from 40,000 to 150,000 years, which does not sound imposing, considering that we have been taught by the war, to reckon with billions. The death, presumably, is caused by debile senility or senile debility. From our conception of death, however, there is inseparable the corpse, the cadaver or some remains. Radio-active matter, however, according to the new theory, has no remains, it is annihilated, which forces us to postulate the existence of some physico-chemical Nirvana.

But, not only is questioned the Ultimacy of Matter, Mr. W. cometh forward, moreover, with the cruel indictment: Matter is no entity at all. Consequently, it

must be a mirage, like the one, poor deluded Peary mistook for Crocker's Land. However, Mr. Whitehorn himself saves us from our embarrassing, not to say, exasperating predicament. He does not dispute that matter is a reality. Thus, matter at one and the same time is Reality and Non-entity. This seeming contradiction at once furnishes us the desired orientation:

"Space exists and does not exist; Matter is something and is nothing." We are on Hegelian grounds, right in the middle of Hegelian Dialectics. "Everything is, what it is, and its opposite, therefore limited and reconciled. In the case of matter, the reconciliation takes the form of movement."

We had been promised a philosophical land, where there flows the milk of empirical knowledge together with the honey of emotional intuition, but Mr. Whitehorn has led us back by a circuitous route through dry theories and dreary calculations into the barren desert of Hegel's Natural Philosophy.

NON-MATERIALITY OF MATTER.

In summing up Mr. W. says: "We have found that mass and extension are derivatives of two most non-material things, energy and motion." We hold this to be a paralogism.

Mindful of Cicero's rule, "Any philosophical discussion should begin with a definition of the subject under discussion," we shall look, first of all, for a definition of energy. Energy is, according to authorities: (1) Condition or attribute by which matter can effect changes in other matter—a very material thing, indeed! (2) A result from the radio-activity of the elemental constituents of the sun, or the aggregate radio-activity of substances, composing the earth.

Kinetic energy is conditioned upon two factors: the ray and the carrying particle. Now, what is the nature of that particle? We maintain it is matter, infinitesimal, if you please, but matter nevertheless. For if not matter, then it must be force, but it can't because the ray is the force. If not matter how can the particle have mass, how could this mass be increased? Even if we were to leave the particle out of consideration how could there be radio-activity without air or ether, which is matter. Nor is any other energy possible without matter. Just as unthinkable as though without brains, or the electrical current without a conductor.

Motion.—There is no motion in itself. Motion presupposes something that moves, be it ether, a solar body, an electric spark, a living creature, a locomotive, a submarine or what not.

Extension.—Extension is defined as space, and space vice-versa as extension and, as a rule, the greatest conceptional divergency, not to say confusion, does prevail in this respect among both philosophers and natural scientists. Extension is (1) the space occupied by a body. (2) Something else, over which there is the disagreement mentioned above. We may, however, obtain the impression of extension by gazing at a homogeneous vastness that stretches forth unbroken far beyond the reach of the eye, if we are, for instance, on the prairie, on the ocean or looking at the sky or down into an abyss—in short, *extension is nature, contemplated in her dimensions.*

ENERGY BECOMES SUPERNATURAL.

"We have thus," continues Mr. W., in his summing up, "by a course of scientific reasoning come to the conclusion that this is not a universe of dead atoms propelled by blind force, but a universe of active energy."

We must beg the writer's pardon, but he has shown nothing of the sort. By what he has told us of the researches, experiments and calculations by Thomson, Rutherford, etc. and of their deductions, by his quotations from them, there is proved a transformation of atoms; the part played by electrons in building up bodies and so on, but these scientists have nowhere said, much less proved, that the electron is intelligent, acts with reason and to a purpose. For this and nothing less is predicted to the energy by Mr. W.'s assertion, that it is not a blind force.

Having endowed the electron with supernatural power, Mr. W. now has to reconcile Science, Philosophy and Theology. Pray how? Theology teaches a Supreme Being, who lives apart from this world, but takes interest in and interferes in human affairs. Does Mr. W. claim as much for his active energy in motion?

It is now our turn to sum up and we can be brief. The writer's proud assertion: "This (the corpuscular) theory has also shed much light upon philosophy in that it has aided us to a truer understanding of the universe," can be substantiated.

Our review has shown on the contrary, that there is not much light and no new light at all. For the philosopher everything has been left by the new theory in statu quo ante. CHARLES DEVIDE.

The Coral Reef Problem and Isostasy

The question of the origin of coral islands (barrier reefs and atolls) has of late been brought to the foreground again by the recent publications of Daly and Davis. According to Darwin's theory a considerable subsidence of the ocean floor over extensive areas is one of the necessary conditions for the formation of barrier reefs and atolls, and although this has well maintained itself in its main principles, yet the great crustal movements required always remain its weakest point. Depressions on such a scale hardly appear likely, though Davis considers such to be indispensable, while Daly upholds an explanation in which subsidence is not put forward as a necessary condition. The latter maintains that in the pleistocene period the storing of large quantities of water in circumpolar ice-caps must have lowered the sea-level in equatorial regions as much as fifty to sixty metres, which caused a corresponding equal lowering of the plane of abrasion and of the final base-level in those regions, whereas after the close of the glacial period a rise of the sea-level must have occurred of about the same amount caused by the melting of those ice-caps. During this last rising of the sea-level or apparent subsidence of the land according to Daly, and on account of it, the formation of the barrier reefs and atolls took place. Such fluctuations of the sea-level caused by the pleistocene glacial periods can hardly be doubted, and the Java shelf sea and Sahul bank of the East Indian Archipelago strongly supports Daly's theory. In both these cases the bottom of the sea at present gives the impression of a submerged, strongly peneplainised land-surface. Thus in Daly's glacial-control theory a fact has been brought forward for the origin of barrier reefs and atolls, which certainly has been of primary importance for the possibility of the formation of many of these types of coral reefs, yet the present author holds that the theory is not sufficient to explain the origin of all barrier reefs and atolls. The theory gives no explanation of true or apparent subsidence of the land of considerably over fifty or sixty meters, which must be admitted to explain the formation of several barrier reefs and atolls such as those in the Pacific, since these rise individually with very steep slopes from abyssmal depths and do not stand connected with others on relatively shallow submarine platforms. True or apparent subsidence of the land of considerable amount is indispensable, in the author's opinion, for the explanation of their origin. The situation at the moment is that such subsidence either true or apparent must be assumed, although considerable diversity of opinion exists as to the amount and cause of this subsidence.

The geographical distribution of coral reefs shows that they occur in regions varying so greatly in geological structure that the causes of subsidence can hardly have been the same in all cases, *e. g.*, crustal warping or folding at the coasts of continents, as in the S. W. Pacific, where subsidence in one region is more or less compensated by an upheaval in another, but other causes for true oceanic regions as in the Central Pacific and Indian Oceans where no compensation by counter-movements has taken place. Subsidence has recently been proved with certainty from borings on the island of Bermuda, which is the only oceanic island in the Atlantic lying within the limits of the geographical distribution of the reef-building corals. The borings prove this island to consist of a volcanic mountain built up of a series of superposed banks of basaltic volcanic material, the probably truncated top of which now lies somewhat more than 75 metres below sea-level. On this truncated cone or platform rests the so-called Bermuda limestone, which is a reef-limestone extending upwards to the surface of the sea. This borehole has revealed the fact that where Bermuda rises from the ocean, submarine volcanoes once were active, of which finally one or more rose above the sea; that these volcanic islands gradually sank away and disappeared under the ocean-level since a period preceding the Eocene; and that their subsidence for a long time has been compensated by the accumulation of successive streams of basaltic lava. The author then considers the cause of such downward movements, and opines that from the theory of isostasy some deductions may be made suggesting a possible or even probable explanation. On this theory the subsidence of the land is a real one caused by the plastic yielding of isostatically non-compensated parts of the terrestrial crust in true oceanic regions under the influence of gravity, and must be expected to occur in all true oceanic volcanic islands. This subsidence may be very considerable and will only stop when such an island has entirely or nearly sunk away into the sima of the ocean-bed, or, rather, will have been recombined with it. The witness to the former existence of such an oceanic volcanic island can be an atoll rising from the bottom of the ocean to the surface. The atoll can only be formed if there is a certain ratio between the rate of subsidence of the island and the upward growth of the reefs, the latter having grown uninterruptedly during the whole period of downward movement. The above hypothesis does not give a satisfactory explanation of the upheavals which occasionally for a time interrupt the process of subsidence. In comparison with the overwhelmingly large number of coral islands for which no rise can be demonstrated, the cases of rising are so few that the author agrees with Darwin's conclusion that the positive movements represents oscillations only in a direction opposite to the general downward trend. The author draws some stringent deductions which serve to test the probability of the theory. In cases where the reef-building corals have been unable to keep pace with the subsidence, corals should be obtained on deep-sea dredging, and a case of this is quoted as occurring in the Cerean Sea. According to this hypothesis deposits formed on the floor of true oceanic regions can never be definitely raised above sea-level and so partake in the building up of continents. Recent work is summarized in an appendix.—Note in *Science Abstracts* on an article by G. A. F. MOLENGRAAFF in *K. Akad. Amst. Proc.*

SCIENTIFIC AMERICAN SUPPLEMENT

Founded 1876

NEW YORK, SATURDAY, OCTOBER 13, 1917

Published weekly by Munn & Company, Incorporated
Charles Allen Munn, President; Frederick Converse Beach, Secretary; Orson D. Munn, Treasurer
all at 233 Broadway, New York

Entered at Post Office of New York, N. Y., as Second Class Matter

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The Scientific American Publications

Scientific American Supplement (established 1876) per year $5.00
Scientific American (established 1845) 4.00
The combined subscription rates and rates to foreign countries, including Canada, will be furnished upon application.
Remit by postal or express money order, bank draft or check.

Munn & Co., Inc., 233 Broadway, New York

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